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EXHIBITING

A CONCISE VIEW

OF THE

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IN

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The following publications have been received:—

The Transactions of the Provincial, Medical, and Surgical Association. Instituted 1832. Volume xii. London and Worcester, 1844. 8vo. pp. 666.

Manual of the Practice of Medicine, the result of fifty years' experience. By W. C. Hufeland, Physician to the late King of Prussia, Professor in the University of Berlin. From the sixth German edition. Translated by C. Bruckhausen, M. S., and R. Nelson, M. D. London, Paris, 1844. 8vo. pp. 630.

The three Cardinal Means of the Art of Healing. By C. W. Hufeland, one of the late Physicians in Ordinary to the late King of Prussia, &c. New York. No date. 8vo. pp. 79.

The Nature and Treatment of Deafness and Diseases of the Ear; and the Treatment of the Deaf and Dumb. By William Dufton, M. R. C. S. London, 1844. Post 8vo. Pp. 118.

On Dysmenorrhœa, and other Uterine Affections in connection with Derangement of the Assimilating Functions. By Edward Rigby, M. D., Fellow of the Royal College of Physicians, &c. &c. London, 1844. Post 8vo. pp. 138.

Outlines of Comparative Anatomy, presenting a sketch of the present state of knowledge, and of the progress of discovery in that Science, and designed to serve as an Introduction to Animal Physiology, and to the Principles of Classification in Zoology. By Robert E. Grant, M. D., F. R. S. L. et E., F. L. S., F. Z. S., M. W. S., &c., Fellow of the Royal College of Physicians of Edinburgh, late Professor of Physiology in the Royal Institution of Great Britain, and Professor of Comparative Anatomy and Zoology in University College, London. Containing Part First, Organs of Relation, or of Animal Life; and Part Second, Organs of Vegetative, or Organic Life. London, Paris, Leipsic, 1841. 8vo. Parts V., VI., and VII., from p. 433 to p. 656.

Report on the Progress of Practical Medicine in the Departments of Midwifery, and the Diseases of Women and Children during the years 1842—3. By Charles West, M. D., Member of the Royal College of Physicians, &c. London, 1844. 8vo. pp. 37.

Observations on the Epidemic Fever of 1843 in Scotland, and its connection with the destitute condition of the Poor. By William Pulteney Alison, M. D., F. R. S. E. and Professor of Practice of Medicine in the University of Edinburgh. Edinburgh and London, 1844. 8vo. pp. 80.

The Chemical and Physiological Balance of Organic Nature. An Essay by M. J. Dumas and M. J. B. Boussingault, Members of the Institute of France. The Third Edition, with New Documents. London, 1844. 12mo. Pp. 156.

Defence of Hahnemann and his Doctrines, including an exposure of Dr Alexander Wood's "Homoeopathy Unmasked." London, Edinburgh, and Glasgow. 1844. 8vo. Pp. 96.

Scarlatina, and its Treatment on Homoeopathic Principles. By Jos. Belluomini, M. D. London, 1843. 8vo. Pp. 31.

On the Application of Prismatic Reflection to the Investigation of Diseases situate in the open Cavities of the Body. By Adam Warden, M. D., F. R. C. S. E. (From the Med. Gazette.) 8vo. Pp. 6. London, 1844.

Fifth Annual Report of the Registrar-General of Births, Deaths, and Marriages in England. Second Edition, revised and corrected. London, 1843. 8vo. Pp. 603.

Forty-Ninth Annual Report of the Glasgow Royal Infirmary. 1843. 4to. Pp. 27.

London Medical Gazette for the Session 1843, 1844. No. 25, March 22. No. 37, June 14, 1844.

The Medico-Chirurgical Review and Journal of Practical Medicine. Edited by James Johnson, M. D. New Series. No. 80, April 1844, being No. 40 of a Decennial Series. London, 1844.

The British and Foreign Medical Review, or Quarterly Journal of Practical Medicine and Surgery. Edited by John Forbes, M. D., F. R. S., &c. No. xxxiv. April 1844. London, 1844.

Weekly Tables of Mortality for the Metropolis. No. 11, Saturday, 16th March 1844. No. 24, week ending Saturday, 15th June 1844.

L'Experience, Journal de Medecine et de Chirurgie Publié par M. Le Docteur J. A. Henroz. No. 350, 14th Mars 1844. No. 361, 13th June 1844.

The New York Journal of Medicine and the Collateral Sciences. Edited by Samuel Forry, M. D. No. vi. Vol. ii. May 1844. New York, 1844.

The Montreal Medical Gazette; being a Monthly Journal of Medicine and the Collateral Sciences. No. 1, April, and 2, No. May, Vol. i.

The Medical Examiner and Record of Medical Science. Edited by Robert M. Huston, M. D., &c. No. 8, Vol. vii. Philadelphia, April 20, 1844.

Provincial Medical and Surgical Sournal. Edited by Robert J. N. Streeten, M. D., No. 1, Vol. i. 3d April 1844—No. 9, 29th May 1844, except No. 5.

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Proposal for establishing a College of Chemistry, for promoting the Science, and its application to Agriculture, Arts, Manufactures, and Medicine. 8vo. Pp. 16. London, 1844.

The Educational and Subsidiary Provision of the Birmingham Royal School of Medicine and Surgery, set forth in a letter to the Rev. Dr Samuel Wilson Warneford, LL. D., Rector of Bourton on the Hill, Gloucestershire, &c. &c. By the Rev. Vaughan Thomas, B. D., Vicar of Stoneleigh, Warwickshire. Oxford, 1843. 8vo. Pp. 61.

Modern Surgical Instruments, chiefly of France and Germany illustrated. London, 1844. Large 8vo. Pp. 16. And 8 Lithographed Engravings.

Dr Boyd's Report as to Lunatics chargeable to the parish of St Mary-le-Bonne. 1844. London, 1844. 8vo. Pp. 16.

The Retrospect of Practical Medicine and Surgery. Being a Half-Yearly Journal, containing a retrospective view of every discovery and Practical Improvement in the Medical Sciences. Edited by W. Braithwaite, Surgeon to the Leeds General Eye and Ear Infirmary, &c. &c. Vol. IX. January—June 1844. London, Edinburgh, and Dublin, 1844. 12mo. Pp. 339.

The British and Foreign Medical Review or Quarterly Journal of Practical Medicine and Surgery. Edited by John Forbes, M.D., F.R.S., &c. &c. No. XXXV. July 1844. London.

The Medico-Chirurgical Review and Journal of Practical Medicine. Edited By James Johnson, M.D., Physician Extraordinary to the late King. New Series, No. 81, July 1844, being No. XLI. of a Decennial Series.

The London Medical Gazette, Session 1843–1844. No. 38, June 21, 1844. No. 50, September 14, 1844.

Weekly Tables of Mortality for the Metropolis. No. 25, Saturday, 22d June 1844; No. 37, Saturday, 14th September 1844.

Provincial Medical and Surgical Journal. Edited by Robert J. N. Streeten, M.D. No. 10, Wednesday, 5th June 1844, to No. 25, Wednesday, 18th September 1844. Worcester and London.

The American Journal of the Medical Sciences. Edited by Isaac Hays, M.D., Surgeon to Wills Hospital, &c. &c. No. XV. New Series. July 1844. Philadelphia.

L'Experience, Journal de Medecine et de Chirurgie. No. 362, 20 Juin 1844; 376, 12 Septembre 1844. Paris.

The Northern Journal of Medicine. Edited by William Seller, M.D., F.R.C. Phys., and T. Lindley Kemp, M.D. No. IV., August 1844.

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1. JULY 1844.

PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*On the alleged Influence of Climate on Female Puberty in Greece.* By JOHN ROBERTON, Manchester.

THE following is offered as another small contribution to the natural history of the female sex, with reference to the age of puberty.

About eighteen months ago a work was published in London by Mr Frederick Strong, Consul at Athens to the Kings of Bavaria and Hanover, with the title "Greece as a Kingdom,"* containing valuable and very interesting accounts of the institutions, the population, productions, commerce, and resources of that country. In the chapter on the soil, climate, and inhabitants, the author has made some curious statements in proof of the precocity of the females; not altogether novel, it is true, for others have said the same things; but as the opinions referred to are advanced as the fruit of his own observation, they acquire, on that account, a claim to particular notice. They are contained in the following passages.

"Nature is so extremely precocious in Greece, that females attain the age of puberty at ten or eleven years, and men at fifteen or sixteen. Young lads of sixteen and seventeen are frequently met with in the villages already married and with families. I am acquainted with a lady of one of the first Athenian families who, though only twenty-five years of age, has already had sixteen children, (eight of them twins), of whom seven are still alive. It may scarcely appear credible in England, but there is now at Athens a venerable grandmamma in the person of a lady not yet twenty-four years old. She was married when eleven years of age, and had a daughter in the course of a year. That daughter married also when scarcely eleven, and has just become a mother.

---

* Greece as a Kingdom; or a statistical description of that country from the arrival of King Otho in 1833 down to the present time; drawn up from official documents and other authentic sources. Dedicated by express permission to His Majesty the King of Greece, by Frederick Strong, Esq., Consul at Athens to the Kings of Bavaria and Hanover. 8vo, London, 1842.

" But female beauty, from its being so precocious, fades quickly, and the freshness and bloom of youth vanish almost as rapidly as they are developed. A married woman of twenty has all the appearance of a middle-aged person of a northern climate, whilst at thirty or thirty-five her face and skin are covered with wrinkles which would do no discredit to a matron of seventy in England or Germany.

" Female beauty bears no proportion to that of the opposite sex.

" Life is prolonged to a very advanced age, particularly in the mountainous districts, and the people retain their faculties of mind and body to the last. Instances of extreme longevity are not at all uncommon; men of ninety and a hundred years being often found able to follow the occupations of the field and the chase."*

Observations such as these, in favour of the influence of climate (for it is to climate that I presume this remarkable precocity is attributed), in promoting the early development of the female constitution, emanating from so respectable a quarter, Mr Strong's book being, in a sense, an official production, and the author, as we learn in his preface, long resident in the country, both deserved respectful attention, and demanded from me especially, (considering the views which I have, from time to time, advanced of a directly opposite bearing), the best inquiry which I might have it in my power to institute. It is true that the latter passage in the above quotation, in which it is stated that life in Greece is prolonged to a very advanced age; that the longevity of the inhabitants is at least equal to what it is in northern Europe; tends to discredit the other statements regarding the early age of puberty; because it ought not to be readily admitted of any race of people, who are acknowledged to live to the average age of the rest of mankind, that the first epoch of life in them can be, as compared with other races, out of proportion, short, since, if such were true, it would follow that one or both of the remaining epochs must, in like manner, be out of proportion, long. If the period from birth to puberty in the Greek female, for example, be actually shorter, by three or four years, as Mr Strong seems to maintain, than it is in more northern latitudes, the period from puberty to the cessation of the catamenia must either be longer, or the last period of all, namely, from the cessation of the catamenia to the end of life, must be so, or the two latter epochs must divide between them the extra length of duration,—a notion which no cautious physiologist would admit without satisfactory evidence; and no such evidence has yet been supplied by any author who has broached the opinion in question.

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* Pages 12 and 13.

The want of beauty,—in different words, the coarseness and plainness of the Grecian women when compared with the other sex, which Mr Strong has noticed, is not owing to their precocity of constitution, as he appears to imagine, but marks a rude and miserable condition of society, in which the weaker sex being, as in similar circumstances is everywhere the case, hardly used, lose the freshness of youth at a comparatively early age, and acquire something harsh and masculine in look and figure, such as is never seen where they are treated with due consideration and tenderness.

Notwithstanding these flaws in Mr Strong's statement, as the opinion he has expressed and the alleged facts were likely on every account to have weight with the public, and also, for other reasons, merited investigation, I wrote to the Rev. Isaac Lowndes, a missionary of the London Missionary Society, in Corfu, inclosing the foregoing extracts from Mr Strong's book, and requesting his opinion as to the credibility of what is therein advanced. As Mr Lowndes is a native of Manchester, reputed for worth and very superior intelligence, and has resided in Corfu or the neighbouring islands for about a quarter of a century, he, it struck me, might be considered extremely well qualified either to answer, or to procure me answers to my inquiries. In a few weeks I had a reply, dated Corfu, January 22, 1843. The portion of the letter from Mr Lowndes having reference to Mr Strong's statements is as follows.

"Your communication, dated December 1, 1842, reached me about a fortnight since, but I did not answer it immediately as the subject is unusual, and I wished, in the first place, to ascertain whether I could entertain any reasonable hope of obtaining for you that information I desire. I have spoken to our medical man, and have every reason to expect correct and circumstantial accounts from him. I purpose also to apply to others, and perhaps may be able to ascertain something from other persons on the subject to which your letter relates. You may rest assured I shall endeavour to meet your wishes; and, whatever may be the result, you may rely on my knowledge of facts, that very early marriages in these countries are by no means very common, for though instances may be produced, they bear a very small proportion in the general scale. There is something so very far from the general state of things in Mr Strong's statement, that I am strongly inclined to think he had been imposed upon by his informers in reference to the accounts he gives of the ages of the venerable matrons of whom he speaks. There is a great propensity in many females in these parts to conceal their age, and to be thought younger than they really are. In addition to which many of them have no register or account of their age, except what is kept at the church, and on such points they often speak much at random.

Further communications I shall forward as soon as I am in possession of them."

A number of months having elapsed, I received the following letter from Mr Lowndes.

Corfu, November 14, 1843.

Dear Sir,—Soon after the receipt of yours, dated December 1, 1842, I acknowledged it, and promised to procure you some information on the subject to which it refers. I now forward you the result of my inquiries in the remarks of three medical men in Corfu relative to your questions. The answers from one of them I only received last week. Dr Usiglio is a Jew, a native of Italy, who has had extensive medical practice in Corfu for many years, both among persons of his own nation and others. Dr Zaviziano is a Greek, a native of Arta, who has also for many years had extensive practice here. Dr Cogevina is a Greek also, a native of Corfu, the son of a medical man in the same place, and he has not only had extensive private practice, but has also under his direction a public general hospital. Their answers to the various questions are quite sufficient to support your side of the question; and on those points in which medical practice is not concerned, I can fully sustain what they have said after a residence of twenty-four years in the Ionian Islands. They all agree in asserting that the statement of Mr Strong is quite preposterous. In a book published in 1822 by Dr Goodison, surgeon in the 75th regiment, a medical officer, who was in these islands when I came to them, it is said, page 222, " the Greeks marry early; there are instances of girls commencing the cares of a family at eleven years of age, many at twelve, and most before sixteen." Now I will venture to say, if Dr Goodison knew these things, he is the only person that ever was acquainted with them. But his statements are perfectly unfounded. I recollect when I first came to these islands, an Englishman, Mr Trelawny, married a Greek girl, who was only thirteen years of age, and it was a subject of very much discussion, as an event so unusual for a female to be married at such an age. Of such another case I have never heard; and when women are married at seventeen or eighteen, it is considered early. There is indeed a very general impression that females in these parts are much more precocious than in England and northern climates in general. I should be disposed to think there may be some ground for this, but observation and experience are quite sufficient to prove that many who have referred to this subject have been greatly mistaken; and no doubt many have taken up the idea from mere report, without once considering how far that report was justified. Dr Usiglio considers females are about one year earlier here than in Italy. You will see the three medical men whom I have consulted differ in their statements in some points; but they have all assured me that the instances of extreme precociousness mentioned by them are very rare, and rather to be regarded as exceptions to what is usual. This, instead of establishing any hypothesis favourable to the statements of Dr Goodison and Mr Strong, is quite opposed to such conclusion. The accompanying table was drawn up by Dr Zaviziano, and this, together with the other remarks, I trust will prove satisfactory to you. Should you wish for anything further, I shall be very happy to do all I can to oblige you. You will see the question, No. 5, I have added, considering it important.—I am, &c.

J. LOWNDES.

P. S. The answers of the medical men were given to me in Italian, and I have translated them into English. None of them know that I have applied to either of the others, so that their opinions are all quite independent.

It is not unlikely that the case to which Dr Cogevina refers, of a girl having a child at twelve years of age, is the one of whom Mr Strong speaks. For it is in Greece, and Dr Cogevina has heard of it.

Answers to Queries by the three physicians referred to, as also a Table by one of them, illustrative of the ages of puberty in a certain number of instances.

Query 1. At how early an age have you known a Greek woman give birth to a child?

By Dr Cesar Usiglio.—" A Maltese born in Corfu at thirteen years of age," (*i. e.* whose mother was thirteen years of age at the period of the said birth.)

By Dr Angelo Cogevina.—" At thirteen years and nine months."

By Dr Anastasius Zaviziano.—" At fourteen years of age."

Query 2. What is the most advanced age at which you have known a Greek woman give birth to a child?

By Dr C. Usiglio.—" When menstruation ceases," (*i. e.* at as late an age as when menstruation ceases.)

By Dr A. Cogevina.—" Fifty-one years."

By Dr A. Zaviziano.—" Generally speaking, when menstruation ceases. I have known one at forty-eight years of age."

Query 3. What is the earliest age at which you have known a female marry?

By Dr C. Usiglio.—" Women from Albania frequently marry early; some at twelve years of age."

By Dr A. Cogevina.—" Some particular cases have occurred at twelve years of age."

By Dr A. Zaviziano.—" One rare case at eleven years of age, the person to whom I referred under the first question. The particular circumstances of this person were the reason of this early marriage."

Query 4. What is the common age of marriage?

By Dr C. Usiglio.—" From fourteen to sixteen years of age."

By Dr A. Cogevina.—" From seventeen to twenty years of age."

By Dr A. Zaviziano.—" After fifteen years of age."

Query 5. At what age does menstruation in general commence?

By Dr Usiglio.—" From twelve to fourteen years of age."

By Dr Cogevina.—" At about fourteen years to fourteen and six months."

By Dr Zaviziano.—" In a very few cases at eleven years of age; more frequently from twelve to fifteen. I have known several of sixteen, seventeen, and eighteen, who had not commenced. One very extraordinary case I have known at nine.

Table supplied by the last-named gentleman.

Corfu, April 27, 1843.

No.	Age at which menstruation commenced.	Age at present.
1	9	20*
2	11	65
3	11	25
4	11	30
5	11	64
6	11	15
7	12	17
8	12	23
9	12	42
10	12	13
11	12	18
12	12	14
13	13	15
14	13	32
15	13	16
16	14	21
17	14	15
18	14	19
19	14	28
20	15	18
21	15	27
22	15	39
23	16	32
24	16	17
25	16	22
26	16	16
27	17	30
28	17	29
29	17	43
30	17	26
31	0	17†
32	18	21
33	18	44
34	18	36
35	0	18‡
36	0	65§

(Signed) Dr Anastasius Zaviziano.

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* To-day I heard of this singular case.

† Not yet.

‡ Not yet.

§ Never.

Query 6. To how late a period does menstruation continue?

By Dr Usiglio.—"Generally (to) from forty to forty-five years."

By Dr Cogevina.—"Till about fifty years of age, but in some not later than forty-two or forty-five."

By Dr Zaviziano.—"Generally from forty-eight to fifty. Some rare cases have occurred as late as fifty or fifty-two. I have, however, known some females stop at thirty."

Query 7. Are there any other facts which you think likely to throw light on these subjects of enquiry?*

By Dr Usiglio.—"I have known a woman bear a child who never menstruated at all. There is a woman in Corfu who had five children at a birth; they all died soon. I have known a woman who lost the menses after having had children, and while she was yet young."

Dr Cogevina.—"The menstrual discharge generally continues six or eight days, and in most cases is copious. There are some particular cases in which menstruation has commenced at eleven or twelve years of age, but these are exceptions to the general rule, (see No. 5 above.) I have heard to-day, (November 11,) of three that menstruated at eleven, and of one in Greece that had a child at twelve."

Dr Zaviziano.—"I knew a female who never menstruated at all, and who never felt any inconvenience on this account. She died last year about sixty-five years of age. She was married three times, but never had a child, or even conceived. I knew another woman (the one mentioned under No. 2 as having had a child at forty-eight,) who, at seventy-five, suckled the child of the daughter-in-law. The mother not being able to suckle it, the old woman took charge of it, and to keep it quiet, put the nipple of her breast into its mouth, and in a few days she had sufficient milk to support it. She commenced with the child when it was about six months old, and continued till it was a year and a half. The child is now a healthy young woman."

The evidence thus supplied concerning the age of puberty in Greece emanates, it will be observed, from three independent witnesses, each in ignorance that any other person was to furnish answers to the queries,—a fact which, seeing that the evidence of the three agrees and harmonizes so well in most respects, must tend to enhance its weight and value. It is worthy of remark, too, that the Rev. Mr Lowndes himself appears to lean to the opinion, that "there may be some ground" for the belief that women in Greece are more precocious than in England and northern climates; as also that one of the physicians, Dr Usiglio, considers that females are about one year earlier in Corfu than in Italy,—a circumstance again giving assurance that the facts have not, at all events, been supplied with any view to support the opinion which I have been led to maintain, namely, that the age of puberty is not affected by climate. The prejudices of the witnesses, so far as they appear, are not in my favour. What conclusion, then, does the evidence which has been presented warrant? Mr Strong affirms that "nature is so extremely precocious in Greece, that females attain the age of puberty at ten or eleven years of age." Does the evidence bear him out?

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* It will be apparent that a portion of the answers to this query are not altogether relevant; but I have thought it best to give the whole, as the facts are not without interest, seeing that they refer to the inhabitants of a region respecting whom we possess extremely little authentic information of a physiological kind.

Only one instance of menstruation is mentioned as having commenced at nine. This is by Dr Zaviziano, and he calls it "a very extraordinary case." No instance is reported as having occurred at ten. The same physician says that menstruation commences "in a very few cases at eleven," while Dr Cogevina is of opinion that cases at eleven and twelve "are exceptions to the general rule."

The thirty-six instances in the table supply, it is true, a large proportion at the ages of eleven and twelve years; but then, on the other hand, there is a still larger and more out of the way proportion at high ages, namely, at sixteen, seventeen, and eighteen years, showing that a considerably greater number of instances would be required to furnish satisfactory data for a calculation of the average age of puberty. Taking the thirty-three instances whose ages are specified, the average age is fourteen. Besides these there are two, the one seventeen and the other eighteen years, in whom the change that indicates puberty had not taken place, which, were they added, would raise the average age to fifteen years. In England the average age is about fourteen years and six months.

Again, the common age at which menstruation commences is stated by one as being from twelve to fourteen years, by another from about fourteen to fourteen and nine months, and by the third from twelve to fifteen. It will further have been remarked, that Mr Lowndes says in his letter, "they (the physicians) have all assured me that the instances of extreme precociousness mentioned by them are very rare, and rather to be regarded as exceptions to what is usual." Such being the evidence of three competent witnesses, it is clear that, instead of supporting, it invalidates the statement of Mr Strong just mentioned, and in truth agrees in all respects, as has been already intimated, with the results obtained in our own country. Without farther referring to those facts illustrative of the period of puberty in England, which were published by me in this Journal in the year 1832, I may quote a sentence from the work of an eminent and accurate observer concerning the age of puberty, as noticed by him in (it may be supposed)

London, the field of his practice, from which a reader would be ready to infer that English women are, at the least, as early as the women of Greece, judging from the answers to the foregoing queries, and Dr Zaviziano's table. "The proper time for the first appearance of the menstrua," says Dr Heberden, "is from the age of twelve years to fifteen. Some show of them has been known in girls of eight or nine years, and even of five; but I never knew an instance of their continuing to return regularly when they began sooner than the tenth year of life."*

The age at which menstruation is said to cease in Greece is quite as late as in England, namely, from forty-eight to fifty; a strong, or rather I would say an irresistible presumptive evidence, were there no other, that puberty is not earlier in the one country than it is in the other. Further,

The instances of early child-bearing given in the answers might perhaps be matched even at the present time in this country; at all events it is likely they could be in Ireland. In every country, in a low state of civilization, such cases are occasionally, perhaps frequently, occurring, and the same will happen in the most highly civilized in times of public disorder, when the ordinary habits and moral restraints in the lower orders of society have been in a measure dissolved and broken up. Thus we are informed by Dr Paris, on what authority is not stated, that during the French Revolution one or two instances occurred of females at eleven, and even below that age, being received in a pregnant state into the hospital, "the Maternité," in Paris.†

The age of marriage, though considerably earlier in the kingdom of Greece than it is in England, would seem, from the above answers, and the testimony of Mr Lowndes, not to be, generally speaking, so early as is stated in the work of Mr Strong, and by several authors who have published Journals of their tours in that region. The table of the ages of marriage in Limerick contained in my last paper, is proof enough that precocious marriage in Europe is not peculiar to its southern shores. Mr Strong's instance of a grandmother at the age of twenty-four, (although, before the reception of such a case as true, it would need the evidence supplied by a register of births, with reference both to the age of the mother and that of the daughter,) might, from known facts, be regarded as just possible in England;* and events of the same kind, quite as strange as this did occur in our own island some centuries ago, as the author of the "Lives of the Berkeleys," whose work I referred to in my former paper, and others besides, have sufficiently testified.

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* Heberden's Commentaries, 3d edition, 1806, page 300.

† Paris's and Fonblanque's Medical Jurisprudence, Vol. i. p. 257. He goes on to say, that "Schurigius states the case of a Flemish girl, who was delivered of a son at the age of nine years; and in the notes to Metzger, several instances are related where conception had occurred under the age of ten." Joubert, as quoted by Dr Gordon Smith, says that he saw a woman at Leetore in Gascony, who bore her first child at nine, a second at eleven, a third at fourteen, and another at sixteen. She menstruated regularly, but was never pregnant after her twenty-first year, though she continued to live with her husband nineteen years afterwards. This author does not attach any greater wonder to such cases than to precocity of judgment, so often met with, and hints that many females at this time of life might conceive were so vile a custom as the marrying of children *generally* practised,—a custom now prevalent in no European community entitled to the name of *civilized*.—Forensic Medicine, p. 494.

It is worthy of notice that the women of ancient Greece do not seem to have been considered as in any sense precocious. On the contrary, the age of marriage was probably as late in Greece as it is in the most refined nations of northern Europe at the present time. The climate being the same then as now, the difference between the modern and the ancient Greeks must, therefore, be sought not in the physical but the moral condition—not in the bodies, but in the low state of civilization of the people. "The time of marriage," says Archbishop Potter, speaking of the ancient Greeks, "was not the same in all places; the Spartans were not permitted to marry till they arrived at their full strength; and, though I do not find what was the exact number of years they were confined to, yet it appears, from one of the sayings of Lycurgus, that both men and women were limited in this affair, that lawgiver being asked the reason of this enactment, said his design was, that the Spartan children might be strong and vigorous, (in a note it is added, 'it seems probable that the usual age for men was thirty and for women twenty years.'") The Athenian laws are said at one time to have ordered that men should not marry till above thirty-five years of age; for human life was divided by Solon into ten (εβδομαδες) weeks, and he affirmed that in the fifth of these men were of ripeness to multiply their kind; but this depended upon the humour of every lawgiver, nothing being generally agreed to in this matter. Aristotle thought thirty-seven good age; Plato, thirty; and Hesiod was much of the same opinion.

Women married sooner than men; some of the old Athenian laws permitted them to marry at twenty-six; Aristotle, at eighteen; Hesiod, at fifteen;† though some think the latter means not at fifteen but in the nineteenth year.

It may well surprise us that so much is attributed to climate by recent writers with reference to a country which was once regarded as the most favoured in the world,—the seat of whatever was greatest in intellectual culture, as well as most elevated and pure in the nature of man. It seems to be forgotten by those who apologise for infantile marriage in Modern Greece on the ground of sexual precocity, that it was in the same region, and in the language of its people, that the principles and duties of the Christian religion were first promulgated, and that it was to Greek converts that an apostle, after enumerating their impure habits indulged in a heathen state, could venture to say, "but now ye put off all these."

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* In a former paper I gave an instance of a patient of the Manchester Lying in Hospital who was delivered in her twelfth year.

† Boyd's Edition of Potter's Archæologia Græca, page 612.

ART. II.—*The Constitution and Diseases of Women. Part I. Containing the Anatomy of the Womb, and the Theory and Diseases of the Catamenia.* By CHARLES BELL, M.D., Fellow of the Royal College of Physicians, Edinburgh, and Assistant Physician to the Edinburgh Maternity Hospital.

IN directing attention to the study of the peculiarities of the female constitution, and to the diseases to which it is liable, it is an object of essential importance to the success of the investigation, that we should make ourselves intimately acquainted with the structure and properties of the uterus; but before proceeding to examine its anatomy in the adult, it will be interesting as well as useful to take a short preliminary view of its condition in the fœtus, and to trace its developement to maturity.

The position of the uterus in the fœtus, like that of the testes, is within the abdomen, and it is found with its ovaries lying on the psoæ muscles, from which situation it does not seem to descend into the pelvis until some time after birth. At this period it is very small, and, according to De Graaf, varies in weight from sixty to ninety grains. In form it is very unlike what it is in the adult, and may be said to consist entirely of cervix. When laid open its cavity is nearly straight, consisting merely of the canal of the uterus, and there is no smooth triangular cavity such as we find corresponding to the body of the organ in its mature condition; but the rugæ of its lining membrane are beautifully and distinctly marked, and more regular than at any other period.*

The developement of the uterus is very gradual, and the period at which it reaches maturity depends not only on climate but on habits. In temperate climates the female organs of generation seldom attain to their perfect form and the full possession of their peculiar properties before the fourteenth year, but in tropical climates this state of perfection is attained at a much earlier period.

The inert state of the uterus in childhood forms a strong contrast to its active condition in the adult. Immediately on attaining maturity it seems to start suddenly into a new state of existence, and whilst it exercises a most powerful influence over the whole animal economy, it seems itself in a great measure unaffected by disease in other organs. This influence is shown in a very remarkable manner during menstruation, and still more in the commencement of pregnancy, and immediately after parturition, when the stomach, head, and mammæ are each or all sympathetically affected, thus indicating an intimate connection between those parts. But the uterus, during its periods of activity, may almost be said to form a distinct and independent system, governed by laws peculiar to itself.* This independence of the uterus is conspicuous in many serious and fatal diseases, and in none more so than in consumption,—many instances occurring in which, while the constitution is hurrying rapidly on to decay, the uterus alone performs its functions with all the vigour of health; the catamenial discharge occurring regularly, and apparently unchanged; and it is by no means a rare occurrence to find patients in the last stage of this fatal malady giving birth to healthy children. A farther example of this seeming independence of the uterus is presented in tedious labour, arising from rigidity of the *os uteri*, or from disproportion between the head of the child and the pelvis of the mother; in many such cases, whilst the strength of the patient is rapidly sinking under long-continued bodily and mental suffering, the uterus continues to act with unabated energy. Another example of this property of the uterus is afforded by the circumstance of impregnation frequently taking place when every function of the body seems suspended from the influence of opium or alcohol. Cases are even narrated in which women have been impregnated when in a state of sleep and quite unconscious.†

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* There is a beautiful preparation in the Museum of the College of Surgeons, Edinburgh, No. 16, which illustrates this appearance of the lining membrane of the uterus in infancy.

See th [illegible]

The remarkable property which the uterus possesses of retaining its integrity during disease in other organs, seems to be one of those wise provisions of the great Author of our being for the preservation of the species, of which we have so many instances in both the animal and vegetable kingdoms. Indeed, if the womb were not thus possessed, as it were, of an independent existence, with all its irritability, it would be liable to be influenced by every serious morbid condition of the system, and the all-important function of generation would be exposed to so many checks and interruptions, that it would be a miracle if any woman attained the full term of pregnancy. But fortunately this is not the case, and miscarriage, though it too frequently produces injury to the general health, principally results from the condition of the womb itself, and excitement of the sexual organs,‡ or from a morbid condition of the fœtus.§

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* The ancients were so much impressed by this apparent independence of the uterus, that they considered it a distinct animal.

† See Beck's Medical Jurisprudence, p. 152.

‡ This opinion receives some support from the researches of Duchatelet, who informs us that miscarriages are common at the end of the first month among prostitutes.—Duchatelet on Prostitution in Paris, Vol. i. p. 276.

§ Dr Simpson, Professor of Midwifery, in a valuable paper published in the Ed.

In recognizing, however, this independent state of the uterus as one of the laws regulating that body, it is not to be concealed that it sometimes partakes of the diseased action affecting other parts of the system, or that it may be influenced by the condition of other organs; indeed, we know that in many cases the functions of the uterus are materially affected by causes external to its system, and that amenorrhœa, sterility, and many other functional disorders, are attributed to deranged action in other organs. But it is probable that this is to be attributed to the circulating system and blood being in a morbid state.

The position of the uterus in the adult.—It is a point of the greatest practical importance to know exactly the situation of the uterus, and we should be led into serious mistakes were we entirely guided in this respect by the position of the uterus in the dead body. After death all the parts are relaxed, and the organs become more or less displaced according to the position of the body. To this circumstance may be ascribed the very different situation of the uterus represented in the two casts in the museum of the College of Surgeons, Edinburgh; the one prepared by the late Mr Wilson, the other by the late Dr Mackintosh; in neither of which is the uterus in the situation in which it is found on examination in the living body. It is, therefore, most important that the ideas obtained of the situation of the uterus by dissection should be corrected by careful examination *per vaginam*, and by the use of the uterine bougie* during life. Indeed it is only by a correct knowledge of the site of the uterus in its normal state that the early existence of displacement, such as prolapsus, retroversion, anteversion, or flexion of the organ on itself, can be ascertained, which conditions, although not necessarily fatal, yet, if not early and skilfully treated, may give rise to sterility, as well as to the most painful and harassing consequences, and may even terminate fatally.

In the unimpregnated state the uterus is, as it were, suspended by its ligaments between the rectum and the bladder, and occupies the axis of the brim of the pelvis. The fundus is nearly on a line with the brim, or slightly above it, and the *os uteri* reaches down nearly to the fourth bone of the sacrum, and can easily be felt by the finger in examination *per vaginam*. This is apparently the natural situation of the uterus; but after repeated gestation or relaxing discharges, it is apt to sink lower in the pelvis, and this may be the case without giving rise to any unpleasant symptoms; although occasionally attended by a con-

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Med. Journal for October 1839, has given several interesting cases of abortion, which were apparently the result of inflammation of the peritoneum in the fœtus.

* This instrument affords great assistance in examining the condition and situation of the uterus. For a description of this instrument and its application see Professor Simpson's Papers in the London and Edinburgh Medical Journal.

siderable increase of the mucous discharge of the vagina. In the unimpregnated state the uterus joins the vagina at an obtuse angle, occupying the space between the bladder and rectum, and giving support to the small intestines. As it rises during gestation, however, it shoots forward, and the intestines gradually fall behind it, so that, in the more advanced stages of pregnancy, the uterus is found to be immediately under the parietes of the abdomen. The connection between the uterus and the vagina is very distinctly marked in the unimpregnated state, when the uterus protrudes into the vagina to the extent of an inch. But during gestation this distinction is gradually diminished, and in the advanced stage, and immediately after parturition, the point of union is very indistinct. To this cause may be ascribed the difference of opinion among modern authors with regard to the hour-glass contraction after labour,—some asserting that the contraction is in the body of the uterus, whilst others believe that it is in the cervix, and that the body of the uterus and the vagina are in a state of relaxation.

The uterus was so called by the ancients from a fancied likeness to a small bottle. Among modern authors, however, it is most generally compared to a flattened pear, to which it bears a considerable resemblance. It is generally about three inches long and two in breadth at the fundus, and gradually tapers towards the orifice, where it does not exceed one inch in diameter; it weighs, according to De Graaf, from one ounce to one ounce and a half, and, even in those who have had children, it seldom exceeds two ounces. (Omnia Opera, p. 238.) In the virgin state it is of a triangular shape, and depressed or flattened in front and behind as if for the accommodation of the bladder and rectum, but during gestation it is much elongated and gradually becomes of an oval form, which increases until it acquires, at the full term of pregnancy, the appearance of an egg. For the convenience of description, authors, at different periods, have divided the womb into distinct parts, distinguished by particular names:—Vesalius divides it into two parts, viz. the cervix and fundus;—Spigelius into the *cervix*, *osculum*, and *fundus*—an arrangement, however, which differs little from the last, as the following passage evidently shows that he considered the vagina to be the *cervix uteri*:—"Dicitur cervix tota illa pars quæ virile membrum in coitu suscipit."* He shows this to have been his meaning still more clearly in another passage, in which he says that the *os uteri* is situated between the cervix and the fundus. Anatomists and writers in the present day divide the uterus into fundus, body, and cervix. The fundus is the superior and broadest part of the uterus, and comprehends all that part above the Fallopian tubes. This portion seems to have been considered by the ancients as the most important part of the organ, being the most frequent seat of the placenta,* and the part in which the menses have their source. This opinion is supported by the circumstance of the fundus being more liberally supplied with blood-vessels than any other part of the organ. Externally it forms almost a semicircle, terminated at each extremity by the Fallopian tube. The body of the uterus is that part extending from the Fallopian tubes to the cervix. The cervix is the narrowest portion of the uterus, and is almost entirely contained within the vagina.

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* Spigelius, De Humani Corporis, p. 280. De Graaf, Oper. Omnia, p. 219.

A perfect knowledge of the *cervix uteri* enables the accoucheur to discover with some degree of certainty the existence and progress of pregnancy; and the French authorities rely much on the information to be gained by examinations of the *cervix uteri* during pregnancy. Accordingly, every teacher of midwifery in Paris gives a separate course of lectures on the "touches," for the purpose of illustrating the changes which take place in this part of the uterus during pregnancy, and, in order that the students may the more readily understand these changes, women in all the stages of pregnancy are hired and indelicately exposed, to enable the teachers to make examinations, and to explain to the pupils the condition of the uterus. Of this mode of acquiring information, however, the state of moral feeling and delicacy of the women in this country, even in the lowest ranks, precludes the English student from availing himself; but this, however useful, is less to be regretted, as such examinations are attended with serious risk to the unhappy females,—inflammation of the vagina and rigidity of the *os uteri* being extremely apt to be produced. A very distressing instance of this kind came under my observation whilst in Paris, in the case of a woman who was in the habit of exposing herself to the examination of the students. I have repeatedly witnessed upwards of a dozen of both sexes subject this wretched creature to tedious examinations, whilst the teacher, who had previously examined her, explained the condition of the parts. This woman had a most tedious and painful labour. I examined her on the second day of her sufferings, after she had already gone through the touche of a dozen students; and I found the *os uteri* rigid, and the soft parts much swollen and extremely painful to the touch. She had a protracted and severe labour, which lasted upwards of three days, and it was with difficulty that her life and that of her infant was saved.

On examination *per vaginam*, in the unimpregnated state, the anterior lip of the uterus appears fuller and more prominent, whilst the posterior is of greater length. This apparent difference is principally owing to the manner in which the *cervix uteri* projects into the vagina, which permits the finger to pass much higher up behind than in front. Great diversity of opinion prevails as to the natural size of the orifice. By some authors it is said to be so small as not to admit a probe;[*] by others that it could admit the *glans penis*.[†] The former opinion seems evidently to have been formed from the state of the *os uteri* when malformed or during gestation, at which time it is entirely closed by a peculiar secretion from the glands of the cervix. My own experience would lead me to say that the *os uteri* in the virgin state will admit the point of the little finger,[‡] and that in those who have born children it is generally a little larger. This is a fact of some consequence, as Dr Mackintosh and others consider that dysmenorrhœa frequently arises from a small *os uteri*, and as it has been supposed that sterility may arise from the same cause. I shall have an opportunity, however, of entering more fully into this subject when treating of the disease itself.

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* Uteri tandem fundus sequitur, qui proprie uterus dicitur, cum omnium partium uterum constituentium nobilissima sit, et in cujus actionem omnes reliquæ conspirant. Vocatur et matrix, quod matris officio fungatur, generendo, et fovendo fœtum. Spigelius, p. 280.

The *os uteri* has also received the name of *os tincæ*, from a supposed resemblance to the mouth of the tench. It lies across the vagina, and its lips are smooth, regular, and are in contact in the virgin state; but, after repeated gestation, they remain separated, and generally become irregular, and sometimes elongated and pointed.[§] This may be the case without any morbid affection.

Fallopian Tubes.—The Fallopian tubes are the two trumpet-like bodies, which arise from the shoulders, as it were, of the uterus, and proceed in a tortuous direction towards the ilium of each side. They are generally about four inches in length, but vary in this respect;[||] they are round and small near the uterus, but gradually expand and terminate in an irregular fringework, which hangs loosely in the iliac fossa. This fringework has received the name of fimbriæ, or *morsus diaboli*. The canal of the tube is extremely narrow, and is lined with a rugous mucous membrane. As it enters the uterus, it is so small that it scarcely admits a bristle, but at its fimbriated extremity it is sufficiently large to receive a moderate sized catheter.[¶] Much difference of opinion exists regarding the structure of the Fallopian tubes. By some authors, who are considered of high authority, they are said to be membranous, and formed only by the outer and inner layer of the uterus.* Others, that they are composed of a spongy fibrous substance, which may be erected like the clitoris by excitement.—Sir Charles Bell considers them totally different in structure from the uterus, but he does not further explain his view of the subject.† The opinion, however, of those who consider the Fallopian tubes to be a continuation of the uterus itself, and of the same structure, is corroborated by Winslow, who says, " Les trompes sont composées de fibres charnues, dont les unes sont longitudinales, les autres obliquement circulaires, toutes garnies et entrelacées d'une autre substance très fine."‡ Again, Haller, in treating of this subject, makes use of the following words :—" Inter duas membranas aliquid spongiosi cellulosi est sed tenerius. Vasorum vero vis magna et forte aliquæ musculares fibræ interponuntur, sed istæ obscuriores."§ These tubes are the medium of communication between the uterus and ovaries; and it is along their canal that the impregnated ovum is conveyed to the uterus. Hence, they are organs of great importance in the female economy, and it becomes a question worthy of investigation, whether or not many instances of sterility are to be ascribed to their being impeded in their action by displacement of the uterus, and by their canal being closed by mucus, or obliterated in consequence of inflammation.

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[*] "Nam inferior ejus angustia, quae os uteri dicitur, in virginibus tam exigua est, ut stylum tenuiorem solummodo excipiat, et nullo conatu vel minimus digitus in eam intrudi possit."—De Graaf, Oper. Omnia, p. 235.

[†] "In coitu contra adeo dilatatur ut penis glandem suscipiat."—Spigelius.

[‡] Since writing the above I find, on consulting Dr Hunter's MSS. Lectures, that he entertained the same opinion. He considers that the *os uteri* was sufficiently large to admit the end of the finger half the length of the nail. Copy of Dr Hunter's Lect. in MSS.

[§] Velpeau Traité des Accouchemens.

[||] Longitudo ejus non semper eadem est; utpote conspicitur 4, 5, 6, 7, et quandoque novem transversos digitos æquare.

[¶] Cruveilhier's Anatomy, Lib. Med. vii. p. 618.

Ligaments of the uterus.—These are four in number; two broad and two round ligaments. The broad ligaments are composed entirely of the folds of the peritoneum passing off from the uterus. They proceed in a horizontal direction from the sides of the uterus, and, from their resemblance to the wings of a bat, they have received the name of *alæ vespertilionis.* In the gravid state their direction is much changed, and they become very oblique in their course. They form a kind of mesentery, giving support to the blood-vessels and nerves which supply the uterus and ovaries, and must have a considerable effect in guiding the ascent of the uterus during gestation. They are, in the virgin state, remarkable principally for the support which they afford to the ovaries, which may be said to be suspended by them.

The round ligaments arise from the body of the uterus, immediately below the Fallopian tubes, and, passing through the abdominal ring, are inserted by numerous fasciculi into the cellular substance of the pubis, and the top of the thigh. Spigelius imagined that these ligaments were hollow, and that they were intended to remove excrementitious matter from the uterus. He seems to have formed this opinion from having found, in one instance, fluid in them. The same opinion, however, was entertained by other ancient authors. Dr Hunter considers that they are entirely composed of vessels, and that their use was not at all understood.[*] Most modern anatomists are of opinion that these ligaments consist of blood-vessels, nerves, and absorbents, bound together by a few muscular fibres. De Graaf denies their muscularity. Sir Charles Bell considered them to be the tendons of the superficial layer of muscles which are spread over the fundus of the uterus, and that their use is to direct the uterus in the commencement of labour. Dionis assigns to them a somewhat similar office *in coitu*, and believes that they keep the uterus *in situ* during the unimpregnated state. That these ligaments perform an important office during gestation can scarcely be doubted, as we find that they increase in size during that period. It is most probable that their use is to guide the ascent of the uterus during gestation, and to prevent its increased weight from pressing injuriously on the intestinal canal, and abdominal blood-vessels and nerves. It is very doubtful if they perform any particular office in the unimpregnated state.

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* De Graaf, Oper. Omnia, p. 340, 342.

† Bell's Anat. Vol. iii. p. 45.

‡ Anat. par Winslow, tome quatrieme, p. 63.

§ Haller's Physiol. p. 531.

The substance and cavity of the uterus.—When the virgin uterus is laid open longitudinally the cut edges present a compact almost cartilaginous surface, but during pregnancy it becomes loose, soft, and elastic in its substance.[†] When closely examined, it is found to consist of three distinct layers, which are separated from each other by cellular membrane. In the virgin state its whole substance is nearly uniform, and is from a quarter of an inch to half an inch in thickness. But much diversity of opinion prevails among authors on this subject. Denman says that "the thickness of the uterus is different at the fundus and at the cervix, being, at the former, usually rather less than half an inch, and, at the latter, somewhat more; and this thickness is preserved throughout pregnancy chiefly by the enlargement of the veins and lymphatics, there being a smaller change in the size of the arteries."[‡] Burns entertains a similar opinion with regard to the difference in thickness between the cervix and fundus. He says, "at the full period of gestation the uterus becomes thinner near the mouth, whilst, at the fundus, it continues the same, or perhaps grows a little thicker, at least where the placenta is attached. Velpeau, however, entertains an opposite opinion, and asserts that the cervix does not exceed two or three lines in thickness, and that the walls of the body are about four lines thick.[‖] Cruveilhier maintains that the walls of the uterus are thinnest where the Fallopian tubes enter, and that they are, at that point, not more than two lines thick. He also states that the parietes of the cervix are thinner than the body, which are usually from four to six lines thick.* The increase in thickness of the walls of the uterus during pregnancy is very trifling,† and cases are recorded in which no increase could be discerned, even where the placenta adhered.‡ Mauriceau, under the impression that the uterus acquired no addition to its substance, imagined that, during gestation, it became thinner in consequence of its being stretched by the increase of its contents.§ This distended condition of the uterus, however, is denied by Dr Hunter, who distinctly says that the uterus is not so completely filled as to put it on the full stretch.‖ The only period at which the uterus may be said to be put on the stretch is during parturition, when at the cervix it is sometimes so stretched as to become as thin as a sheet of writing paper. That the uterus does acquire an increase of substance is proved by the fact, that, after parturition, when freed of the secundines, it is found to weigh 24 ounces,¶ thus becoming twenty-four times heavier than it is in the virgin state. After delivery, the increase in the thickness in the uterine parietes is very remarkable, and gives a striking example of the great contractile power of its muscular fibres. They are usually found an inch in thickness, and Dr Hunter states that he has found them two inches.

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[*] MSS. Notes of Dr Hunter's Lect., p. 63.

[†] Hunter on the Gravid Uterus.

[‡] Denman's Midwifery, Vol. i. p. 75.

[§] Burns' Anatomy of the Gravid Uterus, p. 43.

[‖] Velpeau, Traité des Accouchemens, Vol. i. p. 80.

The first layer of the uterus.—The superficial coat of the uterus, like that of the intestines, is derived from the peritoneum, and does not differ in any respect from that membrane in other parts. It does not form a complete covering to the organ, but is reflected from the lower part of its body to the bladder and rectum, forming what is called the anterior and posterior ligaments of the uterus. It diverges in double layers from the uterus towards the ilium on each side, embracing the ovaries, and forming the broad ligaments, which divide the pelvis into two departments.

The second layer of the uterus.—This is the thickest and strongest of all the coats of the uterus. It gives form to the organ, and is the source of that power which expels the child and secundines, and from the interlacement of its fibres round the blood-vessels, it is of great importance during parturition and after delivery. It is to the regular construction of this coat alone that the accoucheur must trust for the safe accomplishment of labour, and for the protection of his patient against hemorrhage. It is also by a correct knowledge of the force and durability of its action, which can only be acquired by experience, that he must be guided in the management of tedious labour, and as to the precise period when artificial aid becomes necessary. This, however, is a subject of too great nicety and importance to be treated in a cursory manner, and on the present occasion.

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* Library of Medicine, Vol. vii. p. 621.

† Hunter on the Gravid Uterus.

‡ Bell's Anatomy, Vol. iii. p. 45.

§ Mauriceau des Maladies des Femmes Grosses, p. 19

‖ Hunter on Gravid Uterus, p. 2.

¶ Rigby's Midwifery, p. 17.

With regard to the nature of this coat of the uterus, it is scarcely necessary to say more than that it is clearly and distinctly muscular, and that it is regulated by the same laws which govern muscular action in other parts of the body. This was the opinion entertained by Vesalius, and although it has been opposed by some authors, it has been supported by the greater number of the most eminent anatomists and physiologists since his time. Ruysch demonstrated in a very satisfactory manner the muscular fibres of the uterus; but he was mistaken in supposing that the *fundus uteri* was composed of only one muscle, because we find on investigation two such muscles as he describes, which have the Fallopian tubes opening on their centre.* There are evidently several layers of these muscular fibres, which cross each other, forming an intricate and beautiful network, through the interstices of which the blood-vessels pass towards the internal surface of the coat, where they congregate in great numbers, and render it the most vascular part of the organ. The circular fibres seem to prevail in the fundus, whilst in the body and cervix the longitudinal are the most apparent.

The observations, however, of Cruveilhier are opposed to this arrangement of the fibres, as he informs us that the cervix is entirely composed of circular fibres, which intersect each other at acute angles. He found this opinion to be supported by comparative anatomy, and he observed the circular fibres very distinctly displayed in the uterus of a sow which he dissected soon after she had farrowed: the cervix was exclusively composed of the circular fibres and the cornua, which correspond to the body of the uterus in the human female, consisted of two layers of fibres, one external and longitudinal, the other deep seated and circular.†

The third layer of the uterus forms the lining of the proper cavity, and is called the mucous membrane of the uterus. It has hitherto been very imperfectly and vaguely described by authors. By the writings of the ancients,‡ we should be led to suppose that this membrane is like a cullender studded with holes, which are at all times visible to the naked eye; but nothing can be more erroneous than this impression, or less supported by the general appearance of the organ; although quite correct if it refers to its appearance through the microscope.

It is not less remarkable to find that so many modern authors should have remained satisfied with the crude notion that this membrane is merely a continuation of the lining membrane of the vagina,[*] or the epidermis,[†] and that differs from it only in situation; when the most superficial examination shows so decided a distinction in the appearance, as well as the functions of the two organs. Davis, wishing to appear original in his views on the subject, mystifies the matter by stating, that "it is a peculiar propagation from the vascular part of the parenchymatous tissue." A third class agree with Morgagni and Chaussier in denying the existence of the membrane altogether. Whilst this notion is utterly absurd if it refers to the uterus in its ordinary condition, it is not altogether without some foundation in the puerperal state, as there are strong reasons for believing that this membrane forms a part of the secondaries, and is thrown off soon after the birth of the child.

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* Hunter on the Gravid Uterus, Sir C. Bell on the Muscularity of the Uterus, and Mad. Boivin's Treatise on the Diseases of the Uterus.

† Op. cit. p. 621.

‡ Spigelius, p. 280. Winslow, Exposit. Anat. Tome quatrieme, p. 59. Lettre Hist. de l'Acad. Roy. des Sciences, p. 20.

The cavity of the uterus in the unimpregnated state is similar in form to the external organ, and is naturally divided into two portions; the one corresponding with the cervix is of an elliptic form, and is usually called the canal of the uterus; the other corresponding with the body and fundus is triangular, and is called the proper cavity of the organ. This portion, in the natural state, is sufficiently large to hold a common bean at its widest part. The lining membrane in like manner is divided into two distinct portions, and has a raphé occupying the median line, and running perpendicularly throughout its whole extent, being, however, more prominent at the cervix than at the body, where it is scarcely to be distinguished. These two portions of the membrane are not more distinct in appearance than they are in the functions which they perform. The cervical portion resembles the lining membrane of the vagina, it being very rugous, but its rugæ, instead of being transverse like those of the vagina, have their origin from the prominent part of the raphé, and ascend obliquely upwards like the feathers of a quill. These rugæ are remarkably regular in the virgin state, but lose this peculiarity after child-bearing, when they are also less prominent. They have numerous glands situated between them, which, in the commencement of pregnancy, secrete thick yellow mucus, which completely closes the orifice of the uterus, and becomes so hard as to offer considerable resistance to a probe.[‡] From the great regularity of these rugæ, they have been compared to the branch of a palm tree;—hence they have been denominated the *arbor vitæ*. This part of the membrane possesses comparatively little vascularity; and although some authors assert that the menses occasionally flow from its surface, it never assumes that appearance which characterizes the decidua.[§] The object of the rugæ seems to be to give a more secure hold to the plug which closes the orifice of the uterus during pregnancy; and in proof of this being the case, Dr Hunter, when treating of this subject, informs us that, in the early months of pregnancy, this mucus is so securely fixed by the rugæ that it requires several days' maceration before it can be separated.

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[*] Burns's Midwifery.

[†] Haller's Physiology, p. 530.

[‡] Occasionally, however, I have observed it less consistent than the generality of authors lead us to expect.

[§] See Hunter on the Gravid Uterus.

The second part of the membrane differs widely from that which we have been considering, and although, as we have shown, it has been treated by many authors as merely a continuation of the mucous membrane, which lines other open passages, it would be more correct to say that it is a membrane *sui generis*, because there is no other membrane which performs analogous functions. It, however, resembles in many respects the mucous membrane in its general appearance and diseases. It lines the whole of the proper cavity of the uterus, and some authors assert that it extends a short way into the Fallopian tubes. At a distance it appears remarkably smooth and regular in its ordinary state, but when viewed more nearly, it is found to be covered with small villi, in consequence of which it has been compared to fine velvet. It is not improbable that these villi become erected under certain stimuli, and perform a similar office to that of the rugæ of the cervix, by preventing the secretions thrown out on the surface from passing off too hurriedly.

My attention was particularly directed to the consideration of this membrane by the peculiar sensation conveyed to the hand when introduced into the uterus in flooding after delivery. There is then no smooth unctuous surface presented to the hand to glide along, such as we should expect to find from the appearance of the cavity of the uterus in the unimpregnated state, or such as is felt in the case of turning when the hand is placed within the membranes. In place of this, the hand comes in contact with a comparatively rough surface, giving the impression of the muscular fibres being uncovered, and having nothing between them and the hand except a thin layer of serum or blood. This idea is farther supported by the appearance of the internal surface of the uterus on examination by dissection at that period. In place of the smooth membrane like velvet, which lines the uterus in the virgin state, we find a pulpy-looking substance, which may easily be brushed off, leaving the muscular fibres bare, and the open mouths of the vessels exposed, if the uterus be relaxed. This forms a strong contrast to the condition of the lining membrane in the unimpregnated state, when it cannot be separated from the muscular coat unless by long maceration and tedious dissection. If to those circumstances we add the fact, that the lining membrane of the uterus has the same smooth and regular appearance in those women who have born children as it has in the virgin, there can be little doubt that this membrane is thrown off and removed after every pregnancy, as it is impossible to suppose that it could be so much distended, as it invariably must be during pregnancy, and yet when the uterus returns to its natural size, there should be no fold or wrinkle to indicate that it ever had been distended. This circumstance is particularly worthy of notice, as it is so different from what takes place in membranes that have been distended in other parts of the body, and especially the skin where it covers the abdomen, which, after being distended during pregnancy, contracts itself into numerous wrinkles, which form one of the most certain external indications of a woman having born children. The same argument does not apply to the substance of the uterus, because it is composed of muscular fibres, which are acknowledged to have the power of contracting to their natural size after being distended, without being altered in their appearance.

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* Op. cit.

It is satisfactory to find that the opinions here expressed are supported by the investigations and reasonings of the celebrated Dr Hunter, which the following extracts very clearly show. In treating of the internal surface of the uterus, he says, " As to the internal surface of the uterus itself, this is a complex subject which I don't know well how to make plain. *It comes away with the secundines*, and how we cannot rightly conceive of, till we know a little of their nature."* Again, when treating of the membranes of the gravid uterus, he says, " I would just observe, that, besides these three which some call the amnion, and the true and false chorion, or as I call them, amnion, chorion, and decidua, which decidua you will afterwards find to be the *internal lamella of the uterus ;*" ibid. p. 46. When treating of the *membrana caduca*, he farther supports this opinion in the following words : " Now then, gentlemen, as this is such a fleshy, opaque membrane, the only question that remains is this, viz. whether it is the external involucrum of the ovum, or the *internal membrane of the uterus ?* If it is the external involucrum of the ovum it certainly has vessels from the uterus, because we always inject it from the uterus, and this looks as if it was the *internal lamella of the uterus.* It may be that the vessels from the uterus are elongated, but we cannot get women and open them in two days, and then in six days after they are pregnant, therefore we cannot determine from thence absolutely. If it was the outer covering of the fœtus, the Fallopian tubes and *os tincæ* would both come only to the outside ; but as it is the reverse, and they lead to the inside, from this, and its being vascular from the uterus, *it is very plainly the internal membrane of the uterus. Every time a woman conceives, and every time she throws off that conception, the membrane exfoliates from*

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* MSS. Notes of Lectures on Anatomy and Surgery in Library of Royal College of Physicians, Edinburgh, Lect. i. Gravid Uterus.

the uterus. It falls off as stag's horn or bird's feather, when they shade." In alluding to this subject in his treatise on the Anatomy of the Gravid Uterus, Dr Hunter informs us that "the remains of the decidua have been melted down and passed off with the lochia, so that the fasciculated stratum of muscular fibres appear to be bare, and to make the internal surface of the uterus." In another passage, when treating of the *musculus orbicularis* in a woman who died in the ninth month of pregnancy, he says, "I found it everywhere covered by a thin stratum of the decidua, through which the muscular fibres appeared, but with some degree of obscurity; upon rubbing off the under membrane with a cloth, it gave me pleasure to see how exactly the above description agreed with appearances."

Much new and valuable information has been added to our knowlege of the nature and constitution of this membrane by recent microscopic investigation. When examined through the microscope it appears not less distinct from the lining membrane of the vagina in its minute structure than in its general character. It is found to be numerously supplied with tubular glands, from which the secretions are observed to flow in great profusion, especially in the early months of pregnancy, when they have a thick whitish appearance,* and it is covered by a fine layer of epithelium, which is of columnar form, and ciliated from the fundus to the middle of the cervix, thus differing from that membrane covering the vagina and the lower part of the cervix, which is squamous.†

In the early months of pregnancy the microscope discloses still more remarkable differences between this membrane and the other membranes covering the open passages. These distinctions are well described by Dr Sharpey in a valuable paper published in Muller's Physiology, in which he gives an interesting description of this organ in the pregnant state. By the aid of the microscope he has been enabled to carry his investigations into the nature of this organ much further than has hitherto been done in this country, and, by the accuracy of his observations, he has added much important information with regard to its minute structure and uses. But although it is very gratifying to find that the result of those investigations is most favourable to the views which are advocated above on more superficial grounds, it is not a little remarkable that this anatomist, whilst he candidly admits that he has been anticipated in some of his views by Weber, should make no allusion to the opinions of Dr Hunter on the same subject.

On examining the decidua in the early weeks of pregnancy, Dr

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* Muller's Physiology, Vol. ii. p. 1574. This fact is also noticed in Astruc, and several of the older authors.

† Cruveilhier's Anatomy, Library of Medicine, Vol. vii. p. 623.

Sharpey found it in some places one-tenth of an inch in thickness, and obviously consisting of the ***thickened mucous membrane of the uterus.*** "Its surface presented a multitude of small round apertures, (thus corresponding with the description of the internal surface of the uterus of the older writers,) which, on a vertical section, were seen to belong to the tubular glands of the mucous membrane elongated and enlarged. These tubes were lined with white epithelium, which rendered them very conspicuous; they were much waved and contorted towards their deep and, doubtless, closed extremity, and, at various parts, they appeared to be implanted at some depth in the tissue of the uterus." From these, and other observations of a similar kind, he was led to conclude that the apertures on the decidua, which give that membrane its cribriform character, are merely "the openings of the glands of the lining membrane of the uterus, which ***is really converted into the decidua, and discharged from the uterus at parturition.***" He likewise points out, in a very satisfactory manner, that this membrane differs materially from the *decidua reflexa*, which has no apertures on its surface, except just at the angle of reflexion from the *decidua vera.* He verified these observations by comparative anatomy, and they are still further supported by the investigations of Weber.*

By establishing these facts with regard to this membrane, we shall not only confirm the views I have advocated in reference to the true seat of the catamenial discharge, but it will go far to explain the cause of some of the functional diseases, with regard to the nature of which we have hitherto been totally in the dark.

The arteries of the uterus.—The uterus is supplied by four arteries, viz. two spermatic and two uterine, which arise from distinct sources, and are distributed to different parts of the organ. Of these vessels the spermatics are evidently the more important, from their origin, distribution, and great tortuosity.† They arise usually from the aorta immediately below the emulgents; but sometimes they have their origin from the emulgents themselves. They are distributed to the *fundus uteri*, Fallopian tubes, and ovaries. They are remarkably tortuous where they approach the uterus, and this condition of the arteries is much increased during the menstrual period as well as in pregnancy. They form frequent anastomosis with each other and with the uterine arteries. The origin and form of these arteries seem to be intended to secure a pure and uniform supply of blood to the most important part of the uterus, at the same time they indicate an intimate connection between it and the other organs which they supply.

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* Muller's Physiology, Vol. ii. p. 1574.

† It was formerly considered that the tortuous condition of the arteries was for the purpose of retarding the flow of the blood through them ; but it has been most satisfactorily proved recently that this is an erroneous opinion, and that instead of the circulating powers of the arteries being weakened, they are increased. This opinion was supported by Sir C. Bell in his Anat., Vol. iii. p. 466, and it is corroborated by the fact of the artery becoming more tortuous as pregnancy advances.

The uterine arteries arise from the external iliacs, and are principally distributed to the cervix and lower part of the uterus and vagina. The uterine arteries, like spermatics, anastomose frequently with each other, but pursue a shorter course and are less tortuous. On reviewing the arteries of the uterine system, the question naturally occurs, to what end is it that these two sets of arteries differ so widely from each other in form, origin, and insertion, if they are intended to perform exactly the same office? The most plausible theory appears to be that the spermatics are the vessels from which the important secretions of the uterus are derived both in the pregnant and the unimpregnated state; whilst the uterine arteries are those which more particularly support the organic system of the uterus. We are led to this conclusion not only by the spermatics being principally distributed to that portion of the uterus which is lined by the fine secreting membrane, but also by the analogous arrangement of those arteries in the male, in which there can be no doubt of their being the source of the reproductive secretion.

The veins of the uterus.—The veins are more particularly worthy of notice, as it has been asserted by some authors* that they are the source of the catamenial discharge. They are very numerous and tortuous near the uterus, especially near the fundus, and as they recede from it they anastomose freely with each other; thus gradually diminishing in number until they form only four large trunks, which accompany the arteries in their course, and are distinguished by the same names. The spermatic vein of the right side pours its blood into the *vena cava*, whilst that of the left is received by the emulgent vein of the same side. The uterine veins terminate in the internal iliac veins. As the veins of the uterus have no valves, they acquire, during pregnancy, a great size, and are much larger in proportion than the arteries, from which circumstance they have received the name of sinuses. They are always more numerous in the neighbourhood of the placenta.

The lymphatics.—These vessels are very numerous, and seem to pervade the whole substance of the organ; but as their powers are principally called into action in the puerperal state, they are extremely small, and not easily recognized in the virgin. Therefore, in order to see them to advantage, they ought to be injected with quicksilver during the gravid state, or immediately after delivery, at which period they have acquired a great size, and form a complete network covering the whole uterus.[*] They pursue a similar course with the blood-vessels, and terminate in the glands of the vagina, and the iliac and lumbar plexus of glands.[†] It is from this circumstance, it is natural to suppose, that purulent matter is so frequently found in those glands in women who have died of puerperal fever.

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* Haller, Astruc, and Sir C. Bell.

The nerves of the uterus.—The nerves of the uterus are much too numerous and intricate to be satisfactorily described without diagrams. I shall therefore confine my remarks so as merely to point out the intimate connection which exist between this class of organs and the blood-vessels, as it tends to prove the views expressed above in regard to the uses of the different arteries. For a more full illustration of this important system, I would recommend my readers to consult Tiedeman's plates of the nerves of the uterus, and Dr Lee's excellent work on the same subject.

The nerves of the uterus arise from the spermatic and hypogastric plexus, which are composed of branches apparently possessing very different influences. The spermatic plexus consists of branches of the great sympathetic system, and they accompany the spermatic arteries to the uterus and ovaries. From this system of nerves having neither the power of motion, emotion, or sensation,[‡] we are justified in the supposition, that the object of these plexuses is to regulate the functions of the organs to which they are directed. The hypogastric plexuses accompany the arteries of the same name in their course and distribution. They are formed from the branches of the sacral nerves, which have their origin in the continuation of that column of nervous matter which possesses the power of motion and sensation. Hence, the nerves coming from this source may properly be considered as the sensitive nerves of the uterus.

(*To be continued in next Number.*)

ART. III.—*Observations on the Effects of Climate in the Production of Diseases of the Lungs, as shown in the Statistical Reports on the Sickness, Mortality, and Invaliding in the Army.* By ROBERT LAWSON, Assistant Surgeon, 47th Regiment.

THE great prevalence of the diseases of the lungs, and the marked influence of climate in their production, and treatment, render its study highly interesting, and the accurate determination of its effects most important; but until the publication of the Statistical Reports on the Sickness, Mortality, and Invaliding in the army, there were no well-authenticated facts, embracing a sufficiently large number of individuals, to admit of accurate deductions on the subject. The first of these reports exhibited a prevalence of pulmonary disease in the West Indies, nearly as great, and a mortality from it far greater, than is observed either in this country or Canada; and according to a subsequent one, this class of diseases is much more common and fatal in the Mediterranean than was previously supposed. These peculiarities are so marked, indeed, as to have induced the gentlemen who framed the reports to doubt the accuracy of the generally received supposition, that pulmonary diseases are principally attributable to atmospherical vicissitudes, and rapid alternations of temperature, a doubt, which, considering the facts that led to it, must have considerable influence on the opinions of the medical profession in this country. Having served in the West Indies, and Mediterranean, I have had an opportunity of observing the more striking features of the climates of both stations, and have been led to the conclusion, that, even in the West Indies, the inflammatory affections of the lungs are directly attributable to chills, not proceeding, it is true, from great or sudden changes in the temperature of the atmosphere, but still, from atmospheric causes which are equally efficient, and in constant operation. In the following remarks I will endeavour to point out the most important of these causes; and although little absolutely new may be brought forward, still, by grouping the distinguishing features of the climate of each station, and illustrating their influence by the numbers contained in the Statistical Reports, something will have been done towards removing the obscurity in which this subject is at present involved.

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[*] See a beautiful preparation in the Museum of the Royal College of Surgeons, Edinburgh, which exhibits this condition of those vessels very distinctly. This preparation was taken from a woman who died of puerperal fever soon after delivery.

[†] Hunter on the Gravid Uterus.

[‡] Bell's Anat., Vol. i. p. 638.

A chill may be shortly described, as that state of the body which is produced by an abstraction of caloric from the surface, more rapidly than from the state of the health at the time, can be restored by the exercise of its natural functions. This state, in temperate and cold climates, is usually produced by the action of cold, in whatever form it may be applied, and is familiar to every one; but many may, at first, have some difficulty in understanding how it can be caused in a warm climate, where, comparatively speaking, cold is never experienced. When it is considered, however, that, while the temperature of the body is about 98° Fahrenheit, the mean temperature at the equator but little exceeds 80°, the possibility of its occurrence even there is obvious.

The conditions of the atmosphere, which influence the abstraction of heat from the body, are three, viz. its temperature, its motion, and its hygrometric state; and the character of a climate mainly depends on the manner in which these are combined.

When the temperature of the air is low heat is rapidly absorbed from the surface of the body, and it could not preserve the natural temperature were radiation, and the free contact of the air, not prevented by thick clothing; as the temperature increases, however, the quantity of heat produced in the system becomes nearly sufficient to supply the loss, and clothing is proportionately less necessary.

Air, of a given temperature, in a state of motion, deprives a warm body of a much greater quantity of heat than air of the same temperature, at rest. According to Sir John Leslie, the quantity of heat removed from a body by air, in a state of rest, would be doubled by giving it a velocity of about 8 miles an hour, and the additional quantity of heat, thus removed, is proportional to the increased velocity; thus, air, with a velocity of 16 miles, would abstract, in a given time, thrice as much heat; with one of 24 miles, four times; and with a velocity of 32 miles, five times as much, as air of the same temperature in a state of rest. As the wind frequently moves with a velocity equal to, or exceeding, the greatest of those mentioned, its power of abstracting heat from the body, especially within the tropics, where the clothing is generally so slight as to afford very insufficient protection, comes to be a matter well worthy of consideration. An individual, for instance, may be in such a state of health, that, with the dress usually employed in the West Indies, the heat produced in the system may be sufficient to replace that removed from the surface, while he remains in still air; but, should he simply move outside his room into the wind, he would immediately be exposed to a loss of heat, in a given time, thrice or four times as great as what the powers of the system were capable of restoring, the inevitable consequence of which would be the reduction of the temperature of the surface nearly to that of the atmosphere, or, in other words, he would experience a decided chill. Such an occurrence is by no means uncommon in the West Indies, among persons debilitated by previous disease.

The hygrometric state of the air is, next to its temperature, the most important condition which can engage the attention of the physician. Hitherto, though its influence was far too obvious to be entirely overlooked, still its effects have been noticed in a general manner only, and that attention has not been given to it which it deserves. Until a few years since most observers completely neglected the hygrometric condition of the air, though they recorded minutely the fall of rain, deeming the latter a sufficiently accurate index of the quantity of moisture suspended in it; but although the fall of rain undoubtedly affects the quantity of hygrometric moisture, their connection is by no means so close as to allow of the amount of the one being deduced from that of the other, even approximatively.

It is usual, in treating of aqueous vapour, to represent its elasticity by the column of mercury it could support, and, as the absolute weight of vapour that could exist in a given space is always proportional to its elasticity, the quantity which could exist in the same space, at different temperatures, is usually indicated by the corresponding elasticities. The quantity of aqueous vapour which can exist in the atmosphere is very small at low temperatures, but it increases with great rapidity as the temperature advances; for, at the temperature of

16°.3	Fahrenheit, the elasticity is	.100	in.
34.0	- -	.200	„
52.8	- -	.400	„
73.0	- -	.800	„
94.8	- -	1.600	„

or, it is doubled for every increase of 19°.6. The air is seldom or never completely saturated with moisture, and would not begin to deposit it until cooled down more or less: the temperature at which the deposition commences is called the dew point. When air is not fully saturated with vapour, it is always capable of taking up an additional quantity, which is proportional to the difference between the elasticity of vapour at the dew point, and at the temperature of the air; thus, taking the numbers in the above table, were the temperature of the air 34°.0, while the dew point was 16°.3, the additional quantity of vapour the air would be capable of taking up would be represented by .100; and, were the temperature of the air 73°.0, and the dew point 52°.8, it would be represented by .400; or, for nearly the same difference between the temperatures of the atmosphere and dew point, be four times greater in the latter than in the former case.

The quantity of heat necessary for the formation of vapour is the same, whatever may be the sensible temperature of the vapour, and, of course, the quantity necessary for converting any portion of water into vapour is directly proportional to its bulk; hence, in the above examples, the heat required to vaporize as much water as would saturate the air in the former, would be to that, in the latter, as 1 to 4. The heat is usually obtained from two sources. When a body, with a moist surface, is exposed to the atmosphere, the film of air in contact with it parts with a certain portion of its heat, which is immediately employed in converting such a quantity of the water into vapour, as will, with the moisture previously existing in the atmosphere, just saturate the film at its reduced temperature; to this a fresh film succeeds, and the process is continually repeated. When the temperature of the evaporating body is higher than that to which the air is reduced, previous to complete saturation, it gradually yields heat until it attain the same temperature, at which it then remains, so long as the temperature of the air and dew point continue the same. Evaporation proceeds with much greater rapidity in the wind than in still air, though, if the temperature of the air, and dew point, be the same in both cases, the reduction of temperature is also almost identical.

When the evaporating body possesses, within itself, the power of generating heat, as in the case of an animal, the reduction of the temperature of the surface will never be so great as in the instance above-mentioned; but, even in the animal, the absolute quantity of heat removed from the surface, in a given time, will depend on the rapidity of evaporation from it. Though the influence of evaporation from the surface, in preventing the undue accumulation of heat in the animal body, has long been acknowledged, I am not aware that it ever has been referred to, as an active cause in reducing the temperature of the body much below the natural standard, a circumstance of daily, nay hourly occurrence.

Such are the conditions of the atmosphere which lead to the abstraction of heat from the body: they are variously combined in different climates, cold air being the chief refrigerating agent in cold climates, and in warm climates and seasons, evaporation. The injurious action of the latter is very much facilitated by the high temperature increasing the exhaling function of the skin, and thus, by rendering the surface moist, bringing it into the most favourable condition for evaporation to produce its greatest effect. The light clothing, too, employed in warm climates, is readily saturated with perspiration, and then, while it favours the increase of evaporation, affords a very insufficient protection to the body from its immediate effects.

The refrigerating influence of evaporation is not altogether inoperative, even in the coldest weather. We often hear of a cold piercing wind, the effect of which on the sensations cannot be estimated by the indications of a thermometer: such a wind has universally a low dew point, and that portion of air which passes through the dress, having its temperature raised, has its capacity for moisture at the same time vastly increased, its power of abstracting it from the surface is correspondingly great, and the heat removed by the evaporation, being in addition to that removed by a current of air, of a low temperature, the sensation of cold experienced is proportionately acute.

Rain possesses a considerable power of abstracting heat from the body, for, in addition to its immediate refrigerating effect, as soon as the dress becomes damp, it virtually forms an evaporating surface, in intimate connection with the body, and from which it removes a variable quantity of heat, according to the rapidity of the evaporation. The number of rainy days seems to have more effect in producing diseases of the lungs than the actual quantity of rain, and, for the obvious reason, that if a moderate fall of rain occur on two days, it is highly probable that a greater number of persons will get wet than if the whole had fallen on one day alone.

The influence of a climate in producing disease cannot be fairly estimated from its annual, or even from its monthly, means of temperature, or moisture, &c., for, except within the range of the trade winds and monsoons, and along the shores of tropical countries, the winds generally vary so much that the most opposite conditions of temperature and moisture may occur during the same month; and an effect, due to the occasional operation of the extreme, cannot be fully accounted for by that of the mean condition. The same remarks apply to disease, annual means of the prevalence of diseases, and of the resulting mortality, though they may show the general character of certain localities, can, of themselves, throw but little light on the influence of climate in their production, and it is only from a knowledge of the peculiarities of the one, combined with the fitful appearance of the other, that their connection can be satisfactorily established. As the Statistical Reports are within the reach of few, and still fewer may have an opportunity of arranging the information they contain on the different localities occupied by the troops, and the climates to which they are exposed, I have given an abridgment of it here, for the sake of reference, and to facilitate comparison.

Table I.—Showing the annual mean temperature, and the mean temperature of the seasons, at the under-mentioned places.

Station.	Annual mean Tempera.	Mean Temperature of				Dif. Mean Tem. warm. and coldest Months.
		Wint.	Spring.	Sum.	Autm.	
London,* . . .	50°.75	39°.9	48°.1	63°.6	51°.4	26°.4
Montreal, . .	42 .32	16 .3	40 .3	67 .3	45 .4	55 .7
Gibraltar, . . .	68 .42	59 .5	65 .3	77 .5	71 .3	20 .5
Malta, . . .	67 .50	57 .3	63 .5	78 .2	71 .0	25 .0
Corfu, . . .	65 .88	54 .5	60 .8	77 .4	70 .8	29 .5
Bermuda, (St George's,) .	67 .68	59 .7	63 .8	75 .5	71 .5	20 .3
Cape of Good Hope, (Cape Town,)	68 .38	60 .5	66 .0	76 .2	70 .8	18 .5
„ Frontier, (Graham's Town,)	69 .08	59 .0	64 .7	81 .3	71 .3	31 .0
St Helena, (James Town,) .	75 .18	71 .4	71 .3	78 .2	79 .7	11 .3
Mauritius, (Port Louis,) .	73 .83	74 .3	78 .0	82 .5	80 .5	9 .5
Barbadoes, (St Ann's,) . .	81 .08	78 .7	81 .2	82 .2	82 .3	5 .0
Jamaica, (Kingston,) . .	80 .58	78 .5	79 .5	83 .0	81 .3	6 .0
Ceylon, (Colombo,) . .	81 .22	79 .1	82 .2	82 .8	80 .8	5 .8

The climate of London has been assumed as the type of the English climates, not that it accurately represents the mean of those to which troops in this country are exposed, but it is, on the whole, the most convenient, and, being accompanied with a series of hygrometrical observations for the whole period, will be more useful, for the sake of comparison, than any other that could have been selected. The variable nature of the weather, especially during winter and spring, is too well known to require comment.

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* The temperatures for London, in the above table, are from observations at the apartments of the Royal Society from 1831 to 1840. See Summary of Weekly Tables of Mortality in Metropolis for 1842. The temperatures for Montreal for five years, by M'Cord, in London, Edinburgh, and Dublin Philosophical Magazine, Vol. xix. p. 285. Those for Bermuda, by Col. Emmet, for 1837, London and Edinburgh Philosophical Magazine, Vol. xii. p. 43, and Vol. xiii. p. 13. The remainder from the Statistical Reports.

The military stations in Canada are scattered over a great extent of country, and, consequently, present considerable differences in climate. In the Statistical Reports, the climate of the lower province is distinguished for the extreme severity of its winter, and the sudden alternations of temperature to which it is subject. From the end of November till May, the ground remains covered with snow: during this period there is, generally, a clear dry atmosphere, with light north-westerly winds, or calm weather, and the sensation of the cold is by no means so acute as might be anticipated from the low range of the thermometer; but when the wind blows from the north-east, especially with any degree of violence, the intensity of the frost becomes excessive. M'Cord, from five years' observations, states, that at Montreal, the winds from north-west, west, and south-west, amount to 54 per cent., from north to 18, from north-east, east, and south-east to 15, and from south to 13 per cent. When the wind changes to the southward and eastward in winter, the usually serene sky becomes overcast, the atmosphere damp, and a considerable rise in the thermometer takes place, accompanied by thick fogs, and heavy falls of snow. The mean temperature of the upper province, according to tables contained in the Statistical Reports, is 5°.7 higher than that of the lower, though the actual range of temperature is much the same. The winter is considerably shorter than in Lower Canada. During spring, summer, and autumn, westerly winds prevail; during winter north-easterly winds are common, but are much less rigorous than in the lower province.

The climate of Nova Scotia is warmer than that of Canada. The atmosphere, however, is subject to great and sudden alternations of temperature; it is also moist, showers are frequent, and fogs common along the sea coast, throughout the year. The prevailing winds are easterly in spring; southerly or south-westerly in summer and autumn; and north-westerly in winter, when a change to any other quarter is generally followed by a rapid rise in the thermometer, accompanied by much rain or snow. The ground is generally covered with snow from December till the end of March.

The climate of New Brunswick is stated to be much the same as that of Nova Scotia, but less liable to great alternations of temperature. The summer is considered warmer, and the winter rather colder, and of longer duration, than in Nova Scotia.*

The climate of the Mediterranean is considerably warmer than any of those already noticed: the difference between the temperatures of the warmest and coldest months, however, is very considerable, being, at Malta, nearly equal to, and, at Corfu, even greater than at London. Contrary to the popular opinion in this country, too, the weather, though often extremely fine, by no means presents that mild and undeviating character usually attributed to it, but, during winter, and spring particularly, fresh breezes, of a sharp bracing nature, are very common. I have seen the thermometer at Malta stand at 32° for a whole day, with a fresh breeze from the northward; and, even in the latter end of March, after a considerable fall of hail, distinct traces of it were found through the streets of Valetta five hours after it fell. These extremes, it is true, do not often occur, but, though the cold be too slight to produce them, it is often sufficient to affect persons in robust health in a most decided manner, and I do not remember ever to have felt the sensation of cold so acutely in this country, as I have done in Malta during a dry northwesterly or north-easterly wind.

There is much misconception as to the nature of the winds in the Mediterranean; those from north to east are cool, and generally dry; in winter and spring they are absolutely cold. From east to south the winds are more or less moist, and frequently extremely so, with hazy weather, as during the sirocco, and are, generally, from 4° to 6° warmer than the mean temperature of the season: these are not generally accompained by rain. The winds from south to west are soft and mild, and are frequently accompanied with heavy rain, but the absolute quantity of moisture in the air is much less than in the sirocco. Hot winds are occasionally experienced from the south-west, or even west-north-west in summer, but they seldom appear more than a few days in a season. From west to north the winds are much drier and cooler than those from the south-west, and, in this respect, hold an intermediate position between those from the north-east and southwest. During summer the winds are generally slight, and present no very marked character; in autumn the sirocco becomes frequent; in winter and spring, fresh breezes from every point of the compass are common, which occasionally increase to heavy gales: the prevailing winds in these seasons, however, are south-westerly in the former, and north-westerly in the latter. One peculiarity deserving notice here, is, that when a fresh sirocco has blown for two or three days, it generally lulls rapidly, and, after an interval of a few hours, is succeeded by an equally strong breeze from the north-west, which, with regard both to its temperature and dew point, contrasts remarkably with the former.

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* At Dover, New Hampshire, which adjoins the south-west part of New Brunswick, on the average of six years, the mean temperature of the year was 44°.26 Fahrenheit; of winter, 22°.7; spring, 41°.8; summer, 66°.2; autumn, 46°.4. Difference between the mean temperature of the warmest and coldest months, 48°.8. In 1838 the winds were from north to east 86 days; from east to south 65; from south to west 91; from west to north 123. American Almanac for 1840, p. 198–9.

In the following table are given the number of days each wind prevailed at Malta during 1841; and in the second column, the mean frequency of the same winds at Ackworth, according to Mr Howard, from eighteen years' observation.* In both cases the direction was noted once daily.

Direction.	Malta.	Ackworth.
North to east,	63	40
East to south,	65	19
South to west,	85	150
West to north,	149	80
Calm and variable,	3	76
Total,	365	365

These remarks on the climate of the Mediterranean apply chiefly to Malta and its neighbourhood: it seems to present much the same character both at Gibraltar, and the Ionian Islands, though at those stations the physical aspect of the country materially modifies its action.

The rock of Gibraltar rises to a height of upwards of 1400 feet, and extends, in a high continuous ridge, from north to south for about two miles. At the southern extremity of the ridge, the rock sinks rapidly by a succession of flats, which terminate at Europa Point. The town lies at the base of the rock, on its western side, near the north end: from its position it is completely screened from all winds between east-north-east and south-south-east, and winds from the north-east would be but partially felt, that is, though the air in the town would exhibit the character as to heat and moisture due to the wind, still its velocity would be infinitely less than would be found at some distance to windward of, or on either side of the rock. According to observations contained in the Statistical Reports, it appears that, from the average of sixteen years, there are annually at Gibraltar, of easterly winds 184 days, of westerly 177. Snow and ice are rare, but during the winter months the cold is keenly felt by those who have been long resident on the rock.

All the barracks at Gibraltar, with the exception of those at Windmill hill, which can accommodate but a few men, are completely sheltered from all winds between the east-north-east and south-south-east.

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* Report of British Association for 1842. Trans. of the Sections, p. 25.

Malta is a low flat island, which is completely exposed to every wind that blows. Valetta, the chief town, in and around which the troops are quartered, is close to the sea on the north-east side of the island, and is quite open on every side, and consequently, the coldness and variableness of the weather, during winter and spring, are experienced to the fullest extent.

The Ionian Islands are mountainous and rugged, and, like all mountainous countries, they are subject to sudden atmospheric vicissitudes, the extremes of cold and heat, dryness and moisture, tempestuous and calm weather, often succeeding each other in the space of a few hours. The neighbouring mountains of Albania, which are covered with snow for six or seven months in the year, reduce considerably, during winter and spring, the temperature of those islands which lie within their influence. The winds in the Ionian Islands present much the same character as to heat and moisture, (with the exception just noticed,) as in other parts of the Mediterranean ; but owing to the irregularities of the surface, they are much more variable and irregular than at Gibraltar, or Malta, and the military stations are less exposed, or if open in one direction, are comparatively sheltered in most others.

The Bermudas consist of a great number of small low islands, which are exposed in every direction. The annual mean temperature is much the same as that of Malta, but the difference between that of the warmest and coldest months is 4°.7 less. Bermuda is situated to the northward of the outer limit of the north-east trade wind, or in what is called by nautical men *the variables*. During autumn, and winter, south-westerly and south-easterly winds, as damp as the sirocco in the Mediterranean, are common, and, as in the Mediterranean, they are often rapidly succeeded by sharp bracing winds from the north-west or north-east. The variability of the climate, during winter, will be readily understood from the fact, that, during January, February, and December 1837, there were, according to Colonel Emmett, 39 gales.

The troops at the Cape of Good Hope are almost entirely quartered in Cape Town ; those on the frontier occupy several posts which are scattered over a considerable extent of country.

Cape Town is situated on a gravelly plain at the west side of Table Bay, having a gentle ascent towards the foot of three barren precipitous mountains, which, stretching from north-west by the south to north-east, form an amphitheatre, having its front to the bay, which is accordingly open to the north. The annual mean temperature is 68°.38, almost identical with that of Gibraltar; but the difference between the mean temperatures of the hottest and coldest months is only 18°.5, or 2°.0 less than occurs there. The prevailing winds at Cape Town are the south-east* and north-east; the former is most common during summer, and, blowing over the sandy flats between the town and Simon's Bay, is usually sultry; the latter is cool, and often accompanied by heavy falls of rain, and violent gales. South-westerly winds prevail during spring and autumn: these are described in the Statistical Reports as being generally surcharged with moisture, which wraps the summits of the mountains over Cape Town in dense fogs; they also often rapidly descend in tempestuous blasts, causing an immediate reduction of temperature, with an equally sudden transition from an extremely dry, to a damp raw atmosphere.

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* It must be remembered, that, in the southern hemisphere, the winds are com-

Graham's Town is the chief military post in the frontier district of the Cape Colony. Its annual mean temperature is 69°.08, somewhat higher than that of Cape Town; the difference between the mean temperatures of the warmest and coldest months, however, is very much greater, being 31°.0, while that at Cape Town is 18°.5 only. The climate in different parts of the frontier varies very materially; about Graham's Town the country is comparatively open, and the winter nights are sharp, clear, and accompanied by slight frosts, while the summer heat, though sometimes intense, is generally tempered by a cooling breeze. Some of the posts which are situated in valleys, more or less completely surrounded by mountains, do not possess this advantage, but the wind becomes heated by the arid sandy surface of the interior, and the temperature during the summer is excessive. On the Keishkamma and Great Fish rivers, for instance, the thermometer has been frequently known to range for several weeks, from 105° to 110°* in the shade at noon, and from 135° to 140° in the sun; and, even during several months, it has seldom been under 95° at that hour. The thermometer has been known to fall in summer from 110° to 64° in the course of a few hours, and in winter, though often as low as the freezing point at night, it sometimes rises to 70° or 80° at mid-day.

Great diurnal range of temperature is common on elevated table lands, when these are not exposed to trade winds or monsoons, a peculiarity first pointed out by Humboldt; and whenever such great differences exist between the temperatures of the

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pletely opposed to those in the northern; thus, there, the south-east wind corresponds in its origin and character with the north-east, to the north of the equator; and the south-west in the former to the north-west in the latter. The north-east and north-west winds, likewise, to the south of the equator, correspond respectively to the south-east and south-west to the north of it.

* It seems highly probable that these numbers give an erroneous idea of the temperature, for, while the difference between the indications of the thermometer in the shade, and exposed to the sun, is 30° only, at page 4 *b* of the Report, a difference of 60°, under similar circumstances, is recorded at Cape Town. The thermometer must have been insufficiently protected from the sun's direct or reflected heat, or exposed to currents of heated air, circumstances, in general, not sufficiently attended to by observers.

day and night, as a regular feature of the climate, general stillness of the atmosphere may be immediately deduced.

St Helena is an island about 10½ miles long and 7 broad. In some places the surface attains an elevation of 2700 feet. The island is divided by a lofty chain of hills, running in a curved direction from east to west, with several ridges branching off towards the north and south, and forming valleys of various extent, but all extremely contracted. James Town, the capital, where the troops were principally quartered, is situated at the entrance of one of these, to the north-west or leeward side of the island; this valley extends about two miles inland, where it is terminated by a precipice of 300 feet high; its sides, also, are very abrupt, and in some places are elevated 600 feet above its bottom. On one of these elevated points a barrack is situated, but no details are furnished in the Reports as to the number of men who have occupied it. St Helena is situated about the centre of the south-east trade wind, and has, consequently, the wind always from one point. The mean annual temperature at James Town is 75°.18, and the difference between the mean temperatures of the warmest and coldest months is only 11°.3. The mean temperature of the high ground in the interior is considerably lower, and the fall of rain is there unusually great, an important point in the present inquiry, for as the wind must pass over the high land before it reaches the leeward side of the island, the evaporation there, from a wet soil, under a tropical sun, and a diminished atmospheric pressure, is extremely rapid, and very materially increases the quantity of moisture in the air. From the extremely sheltered position of James Town the influence of the trade wind is very little felt; and, indeed, over the whole of the lee side of the island it is very much reduced, while, from the cause above noticed, the moisture of the air must be considerably greater than would be found even to windward of the central chain of hills.

Mauritius lies in latitude 20° south, in the Indian Ocean, within the usual limits of the south-east trade wind, though the wind is sometimes north-east or north. Hurricanes are occasionally experienced. The surface of the island rises rapidly from the coast to the interior, where it forms three chains of mountains, from 1800 to 2800 feet in height, intersecting the country in various directions. Owing to the cooling influence of the south-east breeze, which prevails during most of the year, and which is, to a considerable extent, checked by the mountains, the windward side enjoys a temperature cooler, by several degrees, than the leeward.

Two-thirds of the troops were stationed in Port Louis, the capital, which lies on the north-westerly, or leeward aspect, of the island, and the remainder were detached to various posts on the windward side.

At Port Louis the annual mean temperature was 78°.83, and the difference between the mean temperatures of the warmest and coldest months is only 9°.5,—a difference much smaller than any before noticed, except at St Helena. Port Louis stands on a plain, about two miles in length and breadth, open to the sea, but encompassed on all other sides by lofty mountains that shut out the south-east breeze, which prevails in these latitudes during a great part of the year. There are three small posts to leeward, but they are too trivial to require notice here. The posts on the windward side of the island are all much more open than those to leeward, and are besides fully exposed to the south-east trade; but as the sickness and mortality in them are not separated, in the Report, from that of the stations to leeward, it will be unnecessary to notice them farther.

The Windward and Leeward Islands, and British Guiana, embrace a great number of stations, extending from 6° to 17° north latitude. They all lie within the range of the north-east trade wind, though its influence is, in several, considerably modified by local circumstances; but in none of the stations hitherto noticed is the wind so constant, or its influence so great, or so generally experienced, as in those under consideration. The annual mean temperature of Barbadoes is, according to observations contained in the Reports, 81°.08; but from observations of my own, made with much care, there is every reason to conclude that it cannot exceed 79°.0.* It will be sufficiently accurate, for the present purpose, to assume the temperature of Barbadoes as the type of those of the various stations in the Windward and Leeward command, especially as none of them differ very materially from it.

The atmosphere in the West Indies is generally supposed to be excessively moist, but this opinion is quite erroneous. The trade wind, when it arrives at the West India Islands, though it contains a large quantity of aqueous vapour, is still far from being saturated, and evaporation proceeds rapidly. Owing to the greater stillness of the air during the hurricane season, the dew point is then higher than at other times, but the air, nevertheless, is still considerably short of saturation. This character of the atmosphere, however, is greatly modified by locality, for, in some of the mountainous islands in which much rain falls, and in Guiana, the moisture of the air is considerably greater than in those islands where the fall is less. The following table will place the hygrometrical condition of the air in the West Indies in a clearer light, and will give some idea of the comparative rate of evaporation there, in Bermuda, and in England. The mean maximum temperature of the day is inserted, for comparison with the dew point, to show the greatest ordinary effect on a person subjected to its influence.

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* See Edinburgh New Philosophical Journal, July 1843.

TABLE II. Showing the mean maximum temperature, the mean dew point, (that for Barbadoes at 3 P. M.), and the comparative rate of evaporation in still air, in the different seasons, at London, Bermuda, and Barbadoes.

Station.	Winter.			Spring.			Summer.			Autumn.		
	Mean Max. Temp.	Dew Point.	Comp. Rate of Evap.	Mean Max. Temp.	Dew Point.	Comp. Rate of Evap.	Mean Max. Temp.	Dew Point.	Comp. Rate of Evap.	Mean Max. Temp.	Dew Point.	Comp. Rate of Evap.
London*	43°60	36°33	.068	54°20	42°47	.145	71°10	56°77	.291	56°53	47°90	.121
Bermuda†	68 .50	54 .14	.268	70 .14	57 .23	.260	81 .50	69 .10	.353	78 .50	63 .68	.374
Barbadoes‡	81 .46	67 .58	.336	...	...	.	85 .19	70 .64	.447	84 .57	72 .71	.370

These results show, in the most decided manner, the much greater rapidity of evaporation in the West Indies, and Bermuda than in this country ; but, as in the West Indies at least, there is an almost constant and often a fresh breeze, which vastly increases its amount, they fail in conveying an idea of the full extent to which this occurs.

The barracks occupied by the troops in the Windward and Leeward command are, almost without an exception, situated on the leeward side of the islands, and are generally placed on elevations, which attain, in some cases, the height of 600 or 700 feet. From their elevation, and relative position, they enjoy a constant breeze, notwithstanding that the centre of the island may be occupied by mountains of no inconsiderable magnitude.

Jamaica lies in the same latitude as Antigua, one of the northernmost stations in the Windward and Leeward command. The interior of the island is occupied by mountains, which, in some places, attain an elevation of upwards of 7000 feet. The trade wind does not reach Jamaica, but, in its place, sea and land breezes are experienced all along the coast, and for a distance inland, varying according to the physical aspect of the locality. The sea-breeze, as is generally known, commences in the morning, and gradually freshens till 2 P.M., when it begins to decline; and, in the evening, after a variable interval of calm, the land wind comes on, and continues during the night. I am not aware of any observations on the hygrometrical state of the atmosphere in Jamaica, but, as the air over the sea is nearly stationary, we may fairly conclude that it possesses a higher dew point than that in the Windward and Leeward Islands, where air with a low dew point is brought rapidly from a colder to a warmer latitude, in the general current of the trade wind. The land wind, in those stations which are close to the bases of the mountains, is often cold and disagreeable; at Spanish Town it is stated frequently to reduce the temperature 20°, in the course of a few hours.

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* From summary of weekly tables of mortality in the Metropolis in 1842.

† From Emmett, in Lond. and Edinr. Phil. Mag. loc. cit.

‡ Edinr. New Phil. Journal, July 1843. The averages for the winter, at Barbadoes, include December and January only.

The military stations in Jamaica are chiefly situated on the coast, and are, consequently, exposed to both land and sea breezes: there are three considerable posts, however, Spanish Town, Stoney Hill, and Maroon Town, which are more in the interior, and are scarcely within the influence of the sea breeze, while, at the first, the land wind is experienced in its greatest rigour. The annual mean temperature at Kingston is 80°.58; and its distribution throughout the year does not differ materially from that in the Windward and Leeward Islands.

Ceylon lies in the Bay of Bengal, between about 6° and 10° north latitude. It is 270 miles long and 145 broad. The interior of the island is occupied by a mass of mountains, some of the more elevated portions of which exceed 8000 feet in height above the level of the sea. These, towards the north, sink into a low continuous chain, which extends to the northern extremity of the island. Ceylon is within the range of the monsoons, and derives the chief characteristics of its climate from them. The south-west monsoon, coming from the equator, is warm and loaded with moisture, and the wet season generally coincides with it in duration, in those countries in which it prevails. Its influence is fully experienced all over the west and south coasts of Ceylon; in its transit across the high ground in the interior, it is deprived of much of its moisture, and is felt at Trincomalee as a hot dry wind. The south-west monsoon prevails on the west coast from April or May to October; on the east its duration is a month less. The north-east monsoon is drier than the south-west, but I am not aware of any researches into its hygrometrical state. It is felt over the east and south coasts during its continuance, and more partially, both as to duration and force, on the west coast at Colombo. To the eastward it sets in in November, and continues till February or March, while at Colombo it is seldom experienced with steadiness, except during December and January. In the intervals between the monsoons the winds are variable, but generally blow from the sea during the day, and from the land during the night. On the west coast these winds are common for five months in the year, but on the east and north-east coast, where the intervals between the monsoons are less, they prevail for three only. In other terms, the wind will be, on the average of a year, at

	Colombo.	Trincomalee.
From south-west, .	5 months.	5 months.
north-east, .	2	4
Land and sea breeze,	5	3

At Kandy, which is situated nearly in the centre of the island, at an elevation of 1676 feet above the level of the sea, the monsoons are more equally experienced than on the coast, but the irregularities of the surface cause the wind to appear much more variable.

The chief stations occupied by the troops in Ceylon are, Colombo, Galle, Trincomalee, the two former on the west coast, and the latter on the east, and Kandy in the interior, as already mentioned. The annual mean temperature of the stations on the coast is represented as nearly the same; that for Colombo is 81°.22, almost identical with the annual mean temperature of Barbadoes, as taken from the Reports, and the distribution of temperature throughout the year is much the same. At Kandy the diurnal variation of temperature is greater than on the coast, and the annual mean, owing to the elevation, about 7° less.

Moelmyne is a station in the Tenasserim provinces, on the Burmese coast. This has been occupied by troops for ten years only, but, as the mortality from pulmonary disease there is much lower than at any other station, the statistics of which have been investigated, it is advisable to include it here, as no account of the causes of this class of diseases, or explanation of the manner in which they operate, can be perfectly satisfactory, which will not admit of being applied to any locality, at the same time tallying with the facts which have been observed there as to their prevalence.

Moelmyne lies on the east or left bank of the Saluen River, about 30 miles from its mouth, though the distance from the sea, at its nearest point, does not exceed nine. The cantonment is situated between the river and a ridge of hills, of from 200 to 300 feet high, which run parallel to it, at the distance of a mile. Beyond this ridge an immense alluvial plain extends, between north-east and south-east. The ridge just noticed shelters the station from easterly winds, but it is fully exposed to those from every other point. As in Ceylon, this locality is under the influence of the monsoons. The south-west extends from April to October, and is remarkable for its extreme moisture; the north-east continues nearly all the rest of the year, and is characterized by dryness. There are usually variable winds for about a month, between the cessation of one monsoon and the complete establishment of the other. Thus, south-westerly winds would prevail for six months, north-easterly for four months, and variables for two months in a year. The mean temperature of Moelmyne is much the same as that of Colombo, though the difference between the highest and lowest temperatures, as well as the diurnal variation, are considerably greater. In December, January, and the early part of February, there is a considerable reduction of temperature, particularly at night, and the north-east winds give rise to excessively dense fogs, which envelope the whole cantonment, and are seldom dispelled till the morning be far advanced.

A glance at the facts stated above will shew, that, in many stations, the physical aspect of the neighbouring country must modify, and in some must almost destroy, that particular combination of circumstances in the condition of the atmosphere, which leads to the rapid abstraction of heat from the surface of the body; and it is obvious that any attempt to explain the occurrence of the diseases of the lungs, from the operation of the atmospheric causes alone, which prevail in a given climate, without due regard to the modification they undergo, in the immediate vicinity of the station, must be perfectly futile.

The amount, and kind of shelter, afforded by the features in the vicinity of a station, vary: thus, a hill may shut out a cool dry wind, while the station is perfectly exposed to a wind from some other point, of frequent occurrence, but of less refrigerating power; the result on the health will be marked by a great number of admissions from slight inflammatory affections of the lungs and air passages, with a moderate mortality: in another station, the disposition of the elevations may be such, that they shelter the inhabitants from the more prevalent and moderately refrigerating wind, but at the same time leave them perfectly exposed to a more piercing though less prevalent wind, a change in circumstances that will be followed by a less number of admissions from inflammatory disease than in the former case, but, owing to the more severe nature of the cases, the mortality will be relatively, and perhaps even absolutely, much greater. Again, the surrounding elevations may be so situated as to afford complete, or nearly complete, shelter from all cold winds, while the town or barrack is merely open to those coming from the equatorial regions, which are always mild and moist, and accordingly possess a comparatively small power of abstracting heat; in such a climate inflammatory affections of the lungs are less frequent than elsewhere, and its importance, in the treatment of many pulmonic affections, is fully appreciated.

The immediate effect, on the body, of the abstraction of heat from the surface, is to diminish the organic functions going on there, thus checking the flow of blood to it, and leading to internal congestion. When the cold is applied to an extremity, and is continued, the circulation in the whole limb becomes diminished, and equally leads to internal congestion, as in the former case. This congestion generally affects either the lungs, or air passages, the intestines, or the liver, frequently, in warm climates, the muscular substance, and it is directed to any of them by various circumstances, which may be, and very often are, altogether independent of the refrigerating cause. In what follows, the congestion of the lungs, which is usually the first step in the genesis of pulmonary disease, will be chiefly considered, though it will be necessary to advert to congestion of the intestines and liver, to account for the small mortality from diseases of the lungs on some stations.

The particular kind of affection of the lungs or air passages which results from the abstraction of heat from the surface of the body, is much influenced by its condition at the time, and by the manner in which the heat is abstracted; for instance, when the body is perspiring, if a considerable extent of the surface be exposed to dry air so as to admit of evaporation, or if cold be actually applied, as in bathing, a catarrh will be produced; or, if the individual be in bad health, or fatigued, there may result a severe bronchitis. On the other hand, congestion of the parenchymatous substance of the lungs, and pneumonia, seem to be caused by a longer exposure to the refrigerating cause, and are not so intimately connected with checked perspiration as catarrhs or bronchitis.

The diseases of the lungs are classed under the following heads in the Medical Returns of the Army, viz. pneumonia, hæmoptysis, *phthisis pulmonalis*, *catarrhus acutus*, *catarrhus chronicus*, asthma, *dyspnœa continua*, and pertussis. In the Statistical Reports pleuritis is inserted in addition to the above: there are two other diseases mentioned there, too, which obviously originate in cold, and which might, with great propriety, have been classed with diseases of the lungs, viz. *cynanche trachealis*, and laryngitis: as the admissions from these, however, are so very small, that, in a millesimal ratio, they would become sensible in the second place of decimals only, their omission here is a matter of no importance.

Some variety exists among medical officers in the application of these terms; many comprehending, under pneumonia, not only inflammation of the parenchymatous substance of the lungs, but likewise pleuritis and bronchitis; others include the more severe acute catarrhal affections, also, between which and bronchitis there is no defined limit; others again return pleuritis as a distinct disease. As all these diseases, however, distinctly arise from the operation of the same causes, and often run into each other during their course; and as it is impossible to ascertain from the Statistical Reports the extent to which the practice above-mentioned has prevailed, it will, I think, be better, in what follows, to class them together, rather than, for the sake of greater minuteness, to arrange diseases under different heads, which, in many instances, differ but in name. Considerable variety is observed, likewise, in the application of the terms *phthisis pulmonalis* and *catarrhus chronicus*, many officers calling incipient phthisis by the latter term, as well as those cases which more properly come under it; and, in by far the greater number of fatal cases, of the so-called chronic catarrh, which have occurred within my own knowledge, tuberculous disease of the lungs has been found in a very advanced stage. A similar remark is made in the Report on Ceylon,

(p. 14). Under these circumstances, however distinct the diseases generally may be in themselves, still, as the great majority of the fatal cases prove to be really cases of phthisis, it seems advisable that they should also be associated, as it is perfectly impossible to effect a separation, and if the diseases be taken as they at present stand, the mortality resulting from each must, in many cases, be erroneous. For the sake of brevity, and in order to facilitate reference, I will hereafter designate the former division the inflammatory, and the latter the tuberculous diseases of the lungs.

Though, in some cases of phthisis, the first symptom that attracts the patient's attention is the occurrence of a severe attack of hæmoptysis, still, in this disease, the symptoms are generally sufficiently distinct from those of phthisis to prevent their being confounded; and as the mortality from hæmoptysis is very small, an inconsiderable portion of which, only, can be attributable to phthisis, its omission will be unimportant.

TABLE III.—Showing the ratios per 1000 of annual mean strength of admissions, and deaths from diseases of the lungs at the under-mentioned stations. Also the ratios of the deaths to the admissions, and the ratio of the deaths from the inflammatory, to those from the tuberculous diseases of the lungs, on each.

Stations.	Pulmonary affections generally.			Inflammatory affections of lungs.			Tuberculous affections of lungs.			Ratio of deaths from infl. to those from tub. affect.
	Admd.	Died.	Ra-	Admd.	Died.	Ra.	Admd	Died.	Ra.	
United Kingdom,	148·55	7·73	$\frac{1}{19}$	128·05	1·21	$\frac{1}{106}$	16·95	6·14	$\frac{1}{2\cdot8}$	1 to 5·07
Canada,	148·38	6·73	$\frac{1}{22}$	129·53	1·87	$\frac{1}{69}$	15 90	4·65	$\frac{1}{3\cdot4}$	1 to 2·49
N. Brunswick & N. Scotia,	125·39	7·12	$\frac{1}{18}$	104·28	1·82	$\frac{1}{57}$	16 07	4 97	$\frac{1}{3\cdot2}$	1 to 2·73
Gibraltar,	140·82	5·28	$\frac{1}{27}$	117·30	1·20	$\frac{1}{98}$	17·47	3·95	$\frac{1}{4\cdot4}$	1 to 3·29
Malta,	119·61	6·02	$\frac{1}{20}$	98·14	1·52	$\frac{1}{65}$	16·16	4·17	$\frac{1}{3\cdot9}$	1 to 2·74
Ionian Islands,	89·81	4·79	$\frac{1}{19}$	70·56	1·37	$\frac{1}{52}$	13·54	3·15	$\frac{1}{4\cdot3}$	1 to 2·30
Bermuda,	125·66	8·70	$\frac{1}{14}$	103·67	1·62	$\frac{1}{64}$	18·29	6·92	$\frac{1}{2\cdot6}$	1 to 4·27
Cape of G. Hope, (Cape Town,)	197·65	3·92	$\frac{1}{50}$	82·46	1·10	$\frac{1}{75}$	10·78	2·56	$\frac{1}{4\cdot2}$	1 to 2 33
Frontier,	81·60	2·41	$\frac{1}{34}$	62·14	0·75	$\frac{1}{83}$	15·54	1·36	$\frac{1}{11\cdot4}$	1 to 1·81
St Helena,	60·76	3·38	$\frac{1}{18}$	51·94	0·51	$\frac{1}{102}$	7·28	2·37	$\frac{1}{3\cdot1}$	1 to 4·65
Mauritius,	83·54	5·64	$\frac{1}{15}$	63·75	1·61	$\frac{1}{40}$	15 89	3 84	$\frac{1}{4\cdot1}$	1 to 2·39
Wind. and Leeward Islands,	115·10	10·46	$\frac{1}{11}$	81·73	1·86	$\frac{1}{44}$	28·07	8·16	$\frac{1}{3\cdot4}$	1 to 4·39
Jamaica,	84·49	7·52	$\frac{1}{11}$	61·36	0·64	$\frac{1}{96}$	20·01	6·56	$\frac{1}{3\cdot1}$	1 to 10·25
Ceylon,	69·86	4·08	$\frac{1}{17}$	52·36	0·84	$\frac{1}{62}$	11·92	2·96	$\frac{1}{4\cdot0}$	1 to 3·52
Moelmyne,	69·68	2·05	$\frac{1}{34}$	61·47	1·17	$\frac{1}{53}$	5·13	0·59	$\frac{1}{8\cdot7}$	1 to 0·50

It is obvious, from the numbers in this table, that diseases of the lungs, both inflammatory and tuberculous, prevail to a considerable extent in every climate. The averages in the table are for a whole year, and consequently exhibit the sum of the effects resulting from all causes, which have been in operation during the year, however varied these may have been, in their frequency, or intensity, or modified by local circumstances. It is to be regretted that the materials, from which the influence of season, in the production of pulmonary affections, might have been deduced, are so limited, as, had they extended to all the stations, the connection between the conditions of the atmosphere above noticed, and the greater or less frequency of this class of disease, could have been shown in a much more satisfactory manner.

That the influence of season on the frequency of inflammatory affections of the lungs is very considerable, will be seen from the following statement of the number of deaths from catarrh, bronchitis, pneumonia, and pleuritis, in each season, amongst the civil population of all ages in London, and in Malta; supposing the whole number of fatal cases from these diseases to be 1000. The mean annual mortality from the diseases just mentioned in London (average of the years 1841–42), is 2.43 per 1000 of population, while in England generally, on the average of 1838–39–40, it is only 1.38 per 1000. In Malta the mean annual mortality, from the same diseases, is 1.28 per 1000 of population, or very little less than in England generally. The mortality is distributed as follows, viz. in

	London.	Malta.
During Winter, .	358.3	342.9
Spring, . .	234.8	317.8
Summer, .	162.1	158.1
Autumn, .	244.7*	181.3†

The mean temperature at London for the seasons, in the years in which these results were obtained, is somewhat different from that in Table I., being 40°.1, 51°.6, 64°.6, and 51°.6 for winter, spring, summer, and autumn respectively. It thus appears that the mortality from the inflammatory affection of the lungs, in London, is much greater in winter than during any other season, and during summer is least, while in spring and autumn, the mean temperatures of which are, in this instance, the same, it is nearly equal, and holds an intermediate place between that of winter and summer.

The mean temperature of Malta in winter is 57°.3, yet, in consequence of the frequently boisterous state of the weather which then prevails, and the increased susceptibility to its effects, produced by the higher temperature, and the occasional occurrence of a warm, moist breeze, from the southward, the relative mortality in that season is almost as great as it is in London. The mean temperature of spring is 63°.5, nearly equal to that of the summer in London; but, owing to the great prevalence of strong north-westerly, and north-easterly breezes, the influence of the increase of temperature is nearly counterbalanced, and the mortality remains almost as great as during winter. The mean temperature of summer in Malta is 78°.2, and that season is characterized by gentle breezes of an elevated temperature, and a generally still state of the atmosphere, the effect of which is a great diminution in the mortality from the inflammatory affections of the lungs, which descend even lower than in London. The autumn has a mean temperature of 71°.0, yet as the south-east and south-west winds are then common, and there is generally an absence of strong or cold breezes from the northward, the diminution of temperature is not attended with a material increase of the mortality from pneumonia, &c., such as is produced by the colder, and more variable autumn in this country.

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* From summary of weekly tables of mortality in Metropolis for 1841–42, the averages for London being drawn from two years' returns only, can be considered but approximative. Here, as elsewhere in this paper, the winter is considered to commence in December.

† Statistical Reports on the Mortality, &c. among the troops in the Mediterranean, &c., p. 24 a, average of 13 years.

The obvious deduction from these facts is, that the mere temperature of the atmosphere, in temperate climates, has less influence in the production of the inflammatory affections of the lungs than seems generally supposed; and that much is attributable to the velocity of the wind and its dryness, a point that will be farther illustrated in the following remarks.

The annual admissions per 1000 of mean strength, from diseases of the lungs of every description, among the Dragoon Guards and Dragoons, in the United Kingdom, were 148.55, and the mortality, 7.73. The admissions per 1000 from the inflammatory affections were 128.05, and the resulting mortality, 1.21, or 1 in 106 of the admissions. The mortality amongst the Foot Guards (which are chiefly quartered in London,) from the inflammatory affections, for a similar period, was 2.49; and it is deserving of notice, that the ratio between the mortality among the cavalry generally, which is stationed throughout the country, and that in the Foot Guards, is almost the same as between the mortality amongst the civil population of all ages in England and Wales from the same diseases, and that in the same class in London, being 1.21 to 2.49 per 1000 in the former case, and 1.28 to 2.43 in the latter. The returns for the civil population are for different years, and do not embrace a sufficient period to afford a perfectly unobjectionable average; but, nevertheless, the coincidence is remarkable, and cannot be regarded as fortuitous.

In Canada, the annual admissions from diseases of the lungs of every description were 148.38 per 1000 of mean strength, and the mortality 6.73; the former almost identical with those in this country, the latter rather less. The admissions from the inflammatory diseases, in Canada, were 129.53 per 1000 of mean strength, and the mortality 1.87, or 1 in 69; thus showing the more severe character of the disease in the rigorous climate of Canada. The soldier in Canada wears a much warmer sort of clothing than in this country, and, during cold weather, is not exposed on sentry more than an hour at once, or even half an hour, when the depression of temperature is great; while, in every other climate, the usual period is two hours. Yet, notwithstanding these unusual precautions, the ratio of admissions is as great, and the resulting mortality one-half greater than in this country.

The annual admissions from diseases of the lungs of every description, in Nova Scotia and New Brunswick were about 23 per 1000 less, and the deaths a fraction greater, than in Canada. The admissions from the inflammatory affections alone were 104.28 per 1000, and the mortality 1.82, or 1 in 57 of the admissions, a severity considerably greater than is observed even in Canada. In Nova Scotia and New Brunswick nearly the same amount of warm clothing is employed as in Canada, but the men are always on sentry the full period of two hours. It seems reasonable to attribute the greater severity of the inflammatory pulmonary affections, on these stations, than in Canada, to the longer exposure of the men at once to the cold air, which, though it did not produce so many cases, would nevertheless induce a more severe disease in those who became affected.

The three Mediterranean stations, Bermuda, and the Cape of Good Hope, all lie nearly in the same latitude, north or south of the equator, and their annual mean temperatures are also nearly the same, though its distribution throughout the year differs considerably at the various stations. The prevalence of, and mortality from, diseases of the lungs, too, vary to a much greater extent, than, considering the similarity of temperature, would at first be supposed, especially as the clothing, diet, (except in Bermuda and Gibraltar,) and duties of the troops are much the same in all.

At Gibraltar, the annual admissions from pulmonary affections of every description were 140.82, and the deaths 5.28 per 1000 of mean strength. The admissions from the inflammatory affections alone were 117.30, and the deaths 1.20 per 1000, or 1 in 98 admissions; thus showing the operation of the causes of these diseases, both almost as frequently, and, relatively to the power of resistance of the persons subjected to them, with more severity than in this country.

At Malta, the admissions from pulmonary affections generally were 31.21 per 1000 of mean strength less than at Gibraltar, but the deaths were considerably greater. The admissions from the inflammatory affections of the lungs alone were 98.14 per 1000, and the deaths 1.52, or 1 in 65 of the admissions; thus exhibiting the operation of a less frequent but more intense cause than at Gibraltar. This difference in intensity is clearly explicable, in accordance with the principles already developed, by the fact, that the Rock of Gibraltar, rising abruptly to the eastward of the town, not only shelters it almost completely from all easterly winds, but very materially diminishes the force of those from the westward, while, at Malta, the general lowness of the island offers no obstacle whatever to the wind from any point.

In the Ionian Islands, the mortality from diseases of the lungs generally was only 0.48 per 1000 of mean strength less than at Gibraltar, though the admissions from this class were 50 per 1000 less than at the latter station. The annual admissions from the inflammatory affections alone were 70.56 per 1000 of mean strength, and the mortality 1.37, or 1 in 52 of the admissions, a ratio considerably greater than is found at Malta, and nearly twice as great as at Gibraltar; indeed, the actual mortality exceeds that at Gibraltar by one-eighth of the whole. This peculiarity in the pulmonary affections in the Ionian command is obviously, in a great measure, dependent on the mountainous character of the islands and the neighbouring coast of Albania, which, by sheltering the military stations from prevalent winds, diminish the number of attacks which would otherwise result from exposure to them, but which, at the same time, being covered with snow for several months in the year, lead to cold piercing winds, that produce a much more severe disease than is found to prevail at Malta, or Gibraltar, or even in Canada. I am inclined to think, however, that, in the Ionian Islands, the deterioration of constitution produced by the great prevalence of intermittent and remittent fevers, renders the pulmonary complaints more intractable than they would otherwise be.

The annual admissions from pulmonary affections of every kind, in Bermuda, were 125.66 per 1000 of mean strength, and the deaths 8.70. The admissions are almost exactly the same as in New Brunswick and Nova Scotia, but the deaths exceed those in any station that has yet been examined. The admissions from the inflammatory affections alone were 103.67, and the deaths 1.62 per 1000 annually, or 1 in 64; thus closely approximating, both in the frequency of these diseases, and in their resulting mortality, to what was found in Malta. The similarity in the position and physical aspect of both stations is very great, and their climates closely resemble each other: under such circumstances, the almost perfect coincidence of the effects cannot be looked on as the result of chance, but must be attributed to the full operation of the same atmospheric causes, to which the low flat surface of both islands presented no obstacle.

At the Cape of Good Hope (Cape Town), the annual admissions from pulmonary affections of every description were 97.65, and the mortality only 3.92 per 1000 of mean strength. The admissions, and deaths, from the inflammatory diseases alone, were 82.46, and 1.10 per 1000 respectively, being 1 death in 75 admissions. Here the admissions from pneumonia, &c. are less than at Gibraltar, Malta, or Bermuda, but exceed those in the Ionian Islands; the mortality, too, falls short of that at Gibraltar, the station, of all others in the Mediterranean, which is best protected from cold north-easterly winds. By referring to the brief sketch of the locality of Cape Town, already given, it will be seen that it lies at the foot of Table Mountain, which completely shelters it from all southerly winds, though, at certain seasons, cold damp blasts frequently descend from its summit. The comparative immunity from pulmonary disease of the inflammatory type, at Cape Town, together with the general mildness of the cases, may be readily explained from these facts, in accordance with the principles already laid down.

The aggregate strength of the troops employed on the frontier of the Cape colony amounted only to 6630 men, and, consequently, the numbers are too small to admit of any satisfactory deductions. The annual admissions amongst these, from pulmonary affections generally, were about 16 per 1000 of mean strength less than at Cape Town, and the mortality is also 1.51 less. The admissions from the inflammatory affections were 62.14 per 1000 annually, and the deaths 0.75, being 1 in 80 of the admissions. The small number of admissions, and low rate of mortality, are to be accounted for by the general stillness of the atmosphere over the greatest part of the district in which the troops are employed, for, although it be subject to great diurnal variation of temperature, this alone, when clothing proper to the climate is employed, is comparatively innocuous, unless accompanied by a breeze.

The aggregate strength of the troops in St Helena, during the period embraced in the Statistical Reports, was 5908 only, but the extremely low rate of mortality which occurs there, from diseases of the lungs, renders it advisable to notice it here. The annual admissions from diseases of the lungs generally, at St Helena, per 1000 of mean strength, were 60.76 only, and the deaths 3.38. The admissions from the inflammatory affections alone were 51.94, and the deaths 0.51 per 1000, being one death in every 102 of the admissions, ratios lower than have been observed in any station to which the statistical investigations have been extended; the admissions from the inflammatory affections in Ceylon, and the mortality from the same in Jamaica, are the only instances in which a similar immunity is presented. The physical aspect, and climate of St Helena sufficiently account for the rarity of, and low rate of mortality from these diseases, for, as already stated, though the island lies in the centre of the south-east trade wind, the stations occupied by the troops are more completely sheltered from its influence than any other in the whole world, while both the diurnal and annual range of temperature is very limited.

With regard to temperature, the Mauritius is intermediate between St Helena and the West Indies or Ceylon. It lies in the south-east trade, in the Indian Ocean. As two-thirds of the troops were stationed on the lee side of the island, where they were considerably protected from the influence of the wind, and only one-third were stationed on the windward side, where that was fully experienced, the amount of pulmonary disease, and the consequent mortality, must be somewhat modified, and will not present the characters attributable to either side, in a very decided manner. The annual admissions per 1000 of mean strength, from pulmonary diseases generally, in Mauritius, were 83.54, and the deaths 5.64. The annual admissions from the inflammatory affections alone were 63.75, and the deaths 1.61, or 1 in 40 of the admissions. Here the admissions from pneumonia, &c. are not more numerous than on the frontier of the Cape colony, but the mortality is as high as in the variable climate of Bermuda, and the ratio of deaths to admissions considerably greater than in any station yet noticed, a peculiarity which will be found to occur in the Windward and Leeward Islands to an equal extent.

The Windward and Leeward command is all situated within the range of the north-east trade wind, which is usually brisk from November to July, or nine months in the year; during August, September, and October, light winds and calms, with occasional fresh breezes, are experienced. Most of the barracks are so situated as to be fully exposed to the trade. The annual admissions per 1000 of mean strength, from pulmonary affections generally, in this command, were 115.10, and the deaths 10.46. The admissions, and deaths, from the inflammatory affections alone, were 81.73, and 1.86, respectively, being 1 death in 44 admissions. Thus, with a number of cases not exceeding those occurring at the Cape, the mortality actually equals that in Canada. This high rate of mortality does not depend on the original severity of the disease, which, within the tropics, is never found so violent as in colder latitudes, but is mainly owing to the great liability of persons, convalescent from pulmonary affections, to relapses, on the slightest exposure to the wind, even during the warmest period of the day; a result which, despite the most anxious and unremitting attention of the medical officer, but too often follows some trivial neglect of his patient's.

Jamaica is not within the influence of the trade wind, but has sea and land breezes along the coast, which extend to a distance inland, variable according to the features of the neighbouring country. The annual admissions from every description of pulmonic affection among the white troops in Jamaica were 84.49, and the deaths 7.52 per 1000 of mean strength. The annual admissions from the inflammatory affections alone were 61.36 per 1000, and the deaths 0.64, being 1 in 96 of the admissions, a result which has been exceeded only in this country, St Helena, and Gibraltar, and which, with the comparative rarity of these complaints at St Helena, incontestably proves that a hot climate, *per se*, is not necessarily productive of inflammatory affections of the lungs, or inimical to them when they exist. But, when persons in a warm climate are exposed to an almost constant brisk and dry wind, as in the Windward and Leeward Islands, or Mauritius, the case becomes totally altered: in the former, with a number of admissions from the inflammatory affections of the lungs only one-third greater, the mortality is nearly three times as high as in Jamaica; and in the latter, with a similar number of admissions as in Jamaica, the mortality is two and a half times greater; and, even in Jamaica itself, the mortality from these affections (including negroes), is considerably higher at those stations which are fully exposed to the sea and land breezes, than at those which, being more inland, are less under their influence, or which, lying on the coast, are so immediately backed up by mountains, as to diminish or destroy it.

In consequence of the alternation of the monsoons in Ceylon, and the occasional occurrence of land and sea breezes, the prevalence of, and mortality from the inflammatory pulmonic affections there, ought, in accordance with what has been already stated, to hold an intermediate place between those at stations in which the trade winds are found throughout the year, and those, such as Jamaica, where they are never experienced. The annual admissions per 1000 of mean strength, from pulmonary complaints generally, in Ceylon, were 69.86, and the deaths 4.08. The admissions from the inflammatory affections alone were 52.36 per 1000, and the deaths 0.84, or 1 in 62 admissions; ratios indicating a much less active, and general operation, of the causes of these diseases, than in the Windward and Leeward Islands, though producing a greater mortality than in Jamaica. In Ceylon, the deaths from the inflammatory affections are more numerous in Trincomalee and Kandy, which are more exposed to the north-east monsoon, than Colombo and Galle, and, at the first of which, the south-west monsoon is experienced as a hot dry wind; and, as the results are for white troops alone, they are more satisfactory than those for single stations in Jamaica, which include the deaths, both of white and black troops, at each. The annual mortality per 1000 of mean strength, from the inflammatory diseases of the lungs alone, were at

Trincomalee,	1.86	Colombo,	0.75
Kandy, .	1.09	Galle, .	0.32

No instance can be found which could better illustrate the influence of prevalent winds, and the exposure of a station, in the production of those affections, for, here, in a climate the most equable, and of a high temperature, two stations similar in every respect but that mentioned, are found within a few hundred miles, at one of which the mortality is actually as great as in Canada, while, in the other, it is less than anywhere in the whole world. Such coincidences are not the result of accident; others bearing a close analogy to them have been observed in the West Indies, and they afford the strongest evidence of the justice of the views here advocated, as to the causes of the inflammatory diseases of the lungs.

At Moelmyne the annual admissions per 1000 of mean strength from all pulmonary affections were 69.68, and the deaths 2.05 only. The admissions from the inflammatory affections alone were 61.47 per 1000, and the deaths 1.17, or 1 in 53 of the admissions. Here, with a temperature much the same as in Ceylon, inflammatory diseases of the lungs are found more prevalent, and more severe, than in any of the stations in that island, except Trincomalee, where the mortality is nearly a half greater, and where, it seems probable, had the information been sufficiently detailed, the ratio of admissions would have been found equally high. The greater immunity enjoyed by Moelmyne, can only be explained by the shelter afforded by the high ground in its rear. Something, however, is perhaps due to the circumstance, that the night-duties of the soldier at that station are considerably less than on almost any other occupied by the British army.

It thus appears that the brisk trade wind of the West Indies, or north-east monsoon of the Indian Ocean, leads to as high a rate of mortality from the inflammatory diseases of the lungs, as the cold piercing winds of Canada; but, nevertheless, the facts above stated will, I think, carry conviction to every reasonable mind, that a hot climate is, on the whole, highly favourable to these diseases, and they fully support the opinion, long entertained by medical men, of the advantages to be derived by patients labouring under them by a change to a warmer climate during winter. At the same time it is to be hoped that the distinctive characters of isothermal climates, which have been insisted on, will, by placing their influence in a clearer light, and tracing it, as it were, to its termination, remove some of the obscurity which has hitherto involved this subject, and enable members of the profession to form their recommendations for change of climate with more satisfaction to themselves, and with greater likelihood of advantage to their patients.

The tuberculous affections of the lungs display, in their geographical distribution, irregularities which far exceed those presented by the inflammatory diseases, but, by pursuing a course similar to that already adopted, and tracing the disease on each station to its causes, as far as these can be readily ascertained, much of this seeming confusion will disappear. The annual admissions, and deaths, from the tuberculous affections of the lungs, per 1000 of mean strength, are given in Table III., together with the ratio of the mortality from the inflammatory to that from the tuberculous diseases.

A considerable number of invalids are annually sent home from foreign stations, of whom a portion are affected with pulmonary diseases, which are almost exclusively chronic catarrhs and phthisis. Some of these die on the passage home, and many others soon after their arrival in this country; the remainder are either discharged from the service, or sent to the depots of their regiments, where they contribute not a little to swell the mortality from tuberculous disease of the lungs. In the Statistical Reports the number of invalids who have been discharged from the service for pulmonary diseases are given, but as there is no detail of the diseases, it is impossible to employ them advantageously here; however, they must all have appeared in the hospital books on the station as admissions, and it must be remembered, in comparing the following ratios of mortality from tuberculous disease of the lungs on foreign stations with that at home, that the ratios on the former merely represent the deaths on the station, and do not include those among the invalids, whose diseases have been equally contracted there, but who have been sent from it previous to their termination.*

The abstraction of heat from the surface of the body, by continually exciting, and keeping up a state of inflammatory irritation in the air passages, exercises a very considerable influence in the production of phthisis. Sir James Clark states, "I have no doubt that when the least predisposition to tuberculous disease exists, long-continued irritation of the bronchial membrane leads to the deposition of tuberculous matter in the extreme branches of the bronchi and air-cells." This opinion receives some striking illustrations from the statistics of the tuberculous diseases of the lungs among the troops on different stations.

I have already shown that the frequency of the inflammatory diseases of the lungs, and the amount of the resulting mortality, were proportional to the prevalence and activity of the refrigerating causes relatively to the powers of resistance of the constitution, at the corresponding station. The best numerical value for this complicated cause that can be obtained, for the purposes of comparison, is its effect, the mortality from the inflammatory diseases. Now, by comparing this mortality with that from the tuberculous diseases of the lungs, on those stations where the free development of phthisis is not obviously interfered with by adventitious circumstances, it will be immediately seen whether long-continued bronchial irritation exercises any influence in their production, for, if it do, the ratio between the former and the latter, or, in other words, between the prevalence and severity of the causes of inflammatory affections of the lungs, with the amount of exposure to them, and the mortality from the tuberculous affections, ought to be nearly the same in all. Accordingly, in Great Britain, the annual mortality per 1000 of mean strength, from

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* The numbers discharged from the service for pulmonary complaints contracted on foreign stations, were between 2 and 4 per 1000 of mean strength annually, with the exception of Ceylon, from which the numbers discharged were only 1.2 per 1000, and Malta, from which they amounted to 5 per 1000.

	Inflammatory diseases of lungs.	Tuberculous diseases of lungs.	Ratio of inflamm. to tuberculous.
Among Dragoon-Guards and Dragoons was	1.21	6.14	1 to 5.07
Among Foot-Guards,	2.49	10.91	1 to 4.38

Here, though the mortality from the inflammatory affections in the Foot-Guards be twice as high as in the Dragoon-Guards and Dragoons, the ratio of the deaths from the inflammatory to those from the tuberculous diseases is very nearly the same in both descriptions of force, and, indeed, when it is considered that the numbers invalided from the Foot-Guards, and discharged from the service for disabilities, are also nearly twice as great as in the army generally, many of whom were undoubtedly affected with phthisis, and who, had they been retained in the service until the disease terminated, would have swelled the mortality from it to at least twice the amount of that amongst the Dragoon-Guards and Dragoons.

In extending the inquiry to foreign stations it must be recollected that, in consequence of the number of invalids who are annually sent home, the mortality from phthisis is less than it would be were every case to remain until it terminated. It is impossible to estimate the extent to which these affect the mortality on the station; it can only be assumed to have influenced all to nearly the same amount. At the following stations the mortality per 1000 of mean strength annually was, from

	Inflammatory diseases of lungs.	Tuberculous diseases of lungs.	Ratio of inflammatory to tuberculous.
In Canada,	1.87	4.65	1 to 2.49
In New Brunswick, &c.,	1.82	4.97	1 to 2.73
In Malta,	1.52	4.17	1 to 2.74
In Cape of Good Hope, (Cape Town),	1.10	2.56	1 to 2.33
Frontier Dist.	0.75	1.36	1 to 1.81

Though these localities present great diversity of climate, and likewise a very marked difference in the mortality from the inflammatory diseases of the lungs, the deaths from the tuberculous affections bear almost the same ratio throughout, to those from the inflammatory, on the corresponding station, except on the last, in which the aggregate force under observation was too small to warrant any very decided conclusion. Hence, it may be rationally concluded, that the amount of mortality from the tuberculous diseases of the lungs, at any station, is much less dependent on the absolute temperature, than upon the operation of those atmospheric causes, which produce the purely inflammatory diseases of these organs, and which very often lead, in persons predisposed to phthisis, to that frequent or constant bronchial irritation, which seems to be so commonly the immediate precursor, if not actually the immediate cause, of the deposition of the tubercle.

On many stations, owing to circumstances of local operation, the development of tubercle in the lungs seems to be much facilitated, and in others checked. The chief circumstances which contribute to these results, are, 1. the use of salt provisions; 2. the prevalence and severity of intermittent and remittent fevers; 3. the prevalence and severity of bowel complaints, and affections of the liver.

That the frequent or constant use of salt provisions has a most injurious effect upon the constitution, has long been a matter of notoriety; yet, I am not aware that its influence, in predisposing to phthisis, has ever been clearly established. The following facts, however, will show the extent of its operation on stations where salt provisions have been issued to any amount. The mortality per 1000 of mean strength was from

	Inflam. diseases of lungs.	Tuberc. diseases of lungs.	Ratio of deaths from inflam. to tuberculous.	Number of days salt prov. in a week.
In Gibraltar,	1.20	3.95	1 to 3.29	3
In Bermuda,	1.62	6.92	1 to 4.27	5
In Windward and Leeward Islands,	1.86	8.16	1 to 4.39	5
In St Helena,	0.51	2.37	1 to 4.65	7

Here, as in the previous case, under great differences of climate, and with the greatest variation in the mortality from the purely inflammatory diseases of the lungs, the deaths from the tuberculous affections, relatively to those from the inflammatory, gradually increase as the employment of salt provisions increases, and, in every case, considerably exceed those which took place on stations where either no salt meat was used, or in which it was issued only one day a-week. In the Windward and Leeward Islands, it is true, both fevers and local complaints prevail to a great extent, and considerably modify the results, as will be hereafter shown; but, as these affect them in different ways, the former increasing while the latter diminishes their frequency, the numbers given above will perhaps give as fair a view of the influence of salt provisions, in predisposing to phthisis in the West Indies, as can be desired, and their close agreement with those for Bermuda, where salt rations were issued to a similar extent, is certainly remarkable. In St Helena, too, bowel complaints and affections of the liver were very common, the former causing an annual mortality of 13.9 per 1000 of mean strength, and the latter of 4.0, which must have considerably diminished the number of phthisical cases that would otherwise have occurred there.

The question, as to whether the air of a malarious locality be beneficial either in preventing or curing consumption, seems to have been revived, to some extent, within the last few years, and many persons in France and America, from their own immediate observation, have concluded that it is beneficial. Dr Henen, reasoning from the mortality among the troops in the Ionian Islands, adopted the same opinion. In this country, however, an opposite opinion seems to be more generally entertained. Sir James Clark states, " An attack of ague is much more likely to favour the occurrence of consumption than to prevent it."* Dr Forry, who has written on the climate of America, having based his investigations on the medical statistics of the United States' army, says, " Malaria holds a prominent place among the causes productive of the cachectic condition of the system, which precedes the formation of tubercle."† The mortality from the tuberculous affections of the lungs amongst our troops, in the various malarious localities they occupy, completely accords with these opinions of Sir James Clark and Dr Forry, for, although the absolute number of deaths from these diseases may, in the malarious situation, be less than in a more healthy one, still they will always be more numerous, relatively to those from the purely inflammatory diseases of the lungs, than in the more healthy locality. However, where severe bowel complaints are very frequent, the relation may become inverted. Taking the Ionian Islands and Jamaica, and grouping the stations in each command, according to the lesser or greater mortality from fever, the differential effect due to the malarious locality will be obtained in each, and as nearly as possible under the same atmospheric conditions. In the former command one group contains Corfu, Ithaca, and Cerigo, and the other Santa Maura, Cephalonia, and Zante; in the latter, the first group embraces Fort Augusta, Stoney Hill, Falmouth, Maroon Town, and Lucca, and the other, Up Park Camp, Port Royal, Spanish Town, Montego Bay, and Port Antonio. The annual mortality per 1000 of mean strength in each of these groups was, from

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* Sanative Influence of Climate, p. 60, 3d edit.

† See British and Foreign Medical Review for July 1843, p. 42. I have not had an opportunity of perusing Dr Forry's work.

		Fevers.	Inflam. dis. of lungs.	Tuber. dis. of lungs.	Ratio of deaths from inflam. to tuber. dis.
In Ionian Islands,	1st Group,	8.9	1.57	3.03	1 to 1.93
	2d do.	21.8	0.97	2.97	1 to 3.06
Jamaica,	1st Group,	58.1	0.78	6.92	1 to 8.87
	2d do.	122.1	0.40	7.31	1 to 18.28*

Salt provisions were issued to the troops in Jamaica three days a-week, but, making every allowance for their injurious action, the very decided increase of the deaths from the tuberculous affections of the lungs, relatively to those from the inflammatory diseases, and under great variation in the number of these, as the mortality from fevers increases, affords the most decisive proof of the deleterious influence of malarious localities on consumptive patients, and the results for Jamaica go far to prove that, in such localities, the predisposition to tuberculous disease may be engendered to a very great extent.

Whenever the mortality from bowel complaints and affections of the liver is great, that from the tuberculous affections of the lungs becomes diminished relatively to the deaths from the inflammatory diseases. The reason is obvious, for whenever there is a tendency to bowel complaints, whether from the use of salt provisions, bad water, or the derangement of the functions of the liver, the internal congestion, which follows the depression of the temperature of the surface of the body, will be much more likely to take place in the viscus, the function of which is already in a state of activity or disorder, than in the lungs, which, in a warm climate, are never so fully excited as in a cold one, and, consequently, the resulting inflammation will fall more frequently on the bowels or liver, and less frequently on the lungs. It may be supposed that the occurrence of this vicarious disease would diminish the frequency of the inflammatory affections of the lungs and their mortality, equally with those from the tuberculous affections, but a little consideration will show the fallacy of the idea; for, while the inflammatory disease is generally the result of a single exposure, the tuberculous, according to the principles advocated above, only manifests itself when the exposure is frequent, and the fresh attacks, however slight they may be, equally numerous. Now, it is apparent, that, if in any given number of times an individual is exposed to chills, the resulting congestion affect the bowels in preference to the lungs, say to the extent of one-half, the inflammatory affections of the lungs will simply be one-half less numerous; but when frequent attacks of inflammatory affections are necessary to produce a state of the bronchial membranes favourable to the deposition of tubercle, it is perfectly obvious that if the frequency of these attacks be diminished one-half, by the internal congestion, consequent on the exposure, falling on the bowels instead of the lungs, not only would it occupy a much longer time to induce that state of the bronchial membrane, but, in consequence of this very delay, the natural powers of the constitution would have time to act, and would, in many instances, restore it to a healthy state, and thus prevent the occurrence of phthisis at all. The effect of bowel complaints in diminishing the tendency to phthisis will be evident from the annual mortality, per 1000 of mean strength, from

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* The details of mortality for single stations in Jamaica embrace the deaths of the black troops as well as those of the white; but as the former have not constituted more than one-eleventh of the whole force, and among them the deaths from the inflammatory diseases of the lungs were to those from the tuberculous as 1 to 2.67, it is clear that the general mortality will only be affected to a small extent, and that in diminution of the great excess of tuberculous diseases.

	Fevers.	Bowel complaints.	Inflam. dis. of lungs.	Tuber. dis. of lungs.	Ratio of deaths from inflam. to tuberculous
At Mauritus,	1.7	10.6	1.61	3.84	1 to 2.39
— Moelmyne,	9.2	21.3	1.17	0.59	1 to 0.50
— Rangoon,	53.9	230.0	0.67	1.67	1 to 2.49

Thus, though salt provisions were used at Mauritius from ten to fourteen days a-month; at Moelmyne, every second day for nearly two years; and at Rangoon, for several months without intermission, and though, on the last station, the duty was much greater, and the exposure, and privations of the men, beyond all comparison more severe than on any other, while the mortality from fever was unusually high, yet, owing to the enormous amount of bowel complaints, the deaths from diseases of the lungs, both inflammatory and tuberculous, were remarkably low, and the ratio of the mortality from the inflammatory to that from the tuberculous affections much the same as at Mauritius, and almost as favourable as at the Cape of Good Hope, one of the healthiest stations occupied by the British Army. The aggregate force employed at Moelmyne was 6818 men, and the returns extend over a period of ten years; the aggregate force at Rangoon was only 8004 men, and the period of observation two years. In both instances the force is too small, and the period of observation too limited to give perfectly unexceptionable results, but the general agreement is, nevertheless, satisfactory.*

The extremely small number of deaths from phthisis at Moelmyne has, undoubtedly, also been favoured by the little night-duty the European soldier was there called on to perform; he not having been on guard oftener than two nights in twelve, while, elsewhere, he is frequently four, and seldom less than three nights in every twelve, on duty. The increased exposure, fatigue, and want of sleep, consequent on the more frequent succession of duties, have a very decided influence on the healthiest men, and must exercise a proportionately greater effect on those who are at all predisposed to, or actually labouring under disease.

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* In 1838 five deaths occurred from phthisis at Moelmyne, being more than from phthisis and chronic catarrh together, the previous eleven years.—Report on Moelmyne, p. 9.

Extending the investigation to the Windward and Leeward Islands, and Ceylon, and grouping the stations in each according to the amount of mortality from bowel complaints, the results will not only bear out those already obtained, but will show that where bowel complaints produce considerable mortality, and are at the same time accompanied by severe fevers, the number of deaths from the tuberculous affections of the lungs, relatively to those from the inflammatory, will be less than when the fevers remaining the same, the bowel complaints are more seldom met with. In arranging the West India stations it is necessary to omit Barbadoes, for that being the head quarters of the Command, all invalids were sent there from other islands, previous to embarkation for England, many of whom, labouring under phthisis, must have died, and thus increased the mortality from that disease much beyond what was strictly attributable to the climate of the island. The other stations in the Windward and Leeward Command may then be distributed as follows, viz. in the first group, British Guiana, Trinidad, Grenada, Antigua, and St Kitts, and in the second, Tobago, St Vincent, St Lucia, and Dominica. In Ceylon, Colombo and Galle form one group, and Kandy and Trincomalee the other: it appears probable, however, that several deaths may have occurred from phthisis at Colombo among invalids waiting for embarkation for England, though, according to the Statistical Reports, these could not be very numerous. The annual mortality per 1000 of mean strength, then, will be from

		Fevers.	Bowel comp.	Infla. dis. of lungs.	Tuber. dis. of lungs.	Ratio of deaths from inflam. to tuberculous.
Wind. & Lee. Command,	1st Group,	44.5	11.4	1.57	6.20	1 to 3.95
	2d do.	47.8	38.4	2.45	7.36	1 to 3.00
In Ceylon,	1st Group,	7.5	19.3	0.68	3.84	1 to 5.65
	2d do.	23.1	26.6	1.39	3.11	1 to 2.24

As so much weight has been laid on the influence of salt provisions in predisposing to consumption, it would have been advisable to have shown how far their use affected the health of the navy. It is difficult, however, if not impossible, to come to a satisfactory conclusion on this subject, for, while the ration in the army usually contained one pound of salt meat, in the navy it contained only three-quarters of a pound, and in the latter, also, vinegar and lime juice were issued to prevent its injurious effects; and there is no attempt to estimate the average number of days in a month, on which salt provisions were served out. But, in addition to these, the system of invaliding, in the two services, is so perfectly different, that the "mortality on the station," even with the addition of that in "home hospitals," in the navy, are not parallel to those of the same name in the army, and, consequently, do not fairly admit of comparison. Taking the deaths from the diseases of the lungs in the South American squadron, the aggregate force of which was 17,254 men, and the amount of invaliding for "consumptive diseases of the lungs" much less than in either the Mediterranean or West Indian and North American commands, the annual ratios of mortality, per 1000 of mean strength, were from

Inflammatory diseases of lungs.	Tuberculous diseases of lungs.	Ratio of inflammatory to tuberculous.
0.4	1.5	1 to 3.75

So that, while the mortality from the inflammatory affections of the lungs is very little less than amongst the troops at St Helena, the ratio of the deaths from the tuberculous affections to those from the inflammatory, is intermediate between the ratios for Gibraltar and Bermuda, at the former of which salt rations were issued three days, and at the latter five days a-week, and where the mortality per 1000, from fevers, annually, was 9.3 and 11.0 respectively, while, in the South American squadron, it was only 1.3. Could all the cases which have been invalided in the navy be traced to their termination, I have little doubt but that it would be found, that, relatively to the mortality from the inflammatory affections of the lungs, that from the tuberculous diseases would, under similar circumstances, be as high as in the army.

A very remarkable difference, between the two services, is the much lower rate of mortality from diseases of the lungs, generally, in the navy than in the army, which, it appears to me, may be in a great measure caused by the difference of dress, and the habits of the men, for, while the sailor has a loose woollen or linen frock, which he wears at all times under ordinary circumstances, the soldier, while on parade, has his coatee closely buttoned, and has, besides, thick broad belts crossing both the chest and back; and he is no sooner dismissed than he removes the latter, and very commonly the former also, thus reducing his covering to a thin shirt, which is often quite wet with perspiration from previous exercise. I have often seen severe pulmonary affections produced by this practice.

I have endeavoured, in the above remarks, to point out the most active causes both of the inflammatory and tuberculous forms of pulmonary disease, and thus apply, to a practical purpose, a portion of the extremely interesting and varied information contained in the admirable Statistical Reports on the Health of the Army, with what success others must decide; but the extraordinary agreement of the results, obtained in very different climates, and in places very distant from each other, cannot be fortuitous. Some of the most active causes admit of remedy, and several have already been remedied, but others are inseparable from the military profession, though all may be considerably diminished by care and attention on the part of the medical and other officers, under whose immediate care the soldier is placed.

In applying the results obtained from the statistics of pulmonary disease, in the army, to the practice of civil life, it will be obvious that that class of persons in this country, whose means enable them to take advantage of climate, in the treatment of their complaints, will, by proper care, always be able to secure many of the advantages of a change to a warm climate, without, at the same time, subjecting themselves to those disadvantages under which the soldier laboured, and which have contributed so much to increase the mortality in the army. But I trust I have shown that the causes of pulmonary diseases are sufficiently common in many warm climates, to render medical men cautious in their recommendation of certain localities, and particular in their injunctions to their patients to avoid all unnecessary exposure on their arrival in the place ultimately selected.

ART. IV.—*Remarks on the Epidemic Fever of* 1843. By DAVID SMITH, M.D., Glasgow. (Read before the Glasgow Medical Society.)

THE history of epidemic fever is exceedingly interesting, not more from the continually varying character of the disease, than from the theories regarding it which have prevailed from time to time. It would, however, be entirely out of place, as well as useless, in such a paper as the present, to enter upon this extensive field of controversy; since, to mention the names of those who have employed distinguished talent and unwearied industry on this topic, would be but to furnish a muster roll of the great ones in physic; and to review, even in the most cursory manner, each successive theory, would only show that the doctrine of yesterday has never been that of to-day. This continued change of opinion may have arisen from various causes, and, perhaps, the greatest of these originates in overlooking the fact, that fever, during different epidemics, presents in each often very different characteristics, and that, at all times, "much depends upon the class of people among whom a physician practises, and the period of the disease at which he generally sees his patient."[*] To reason correctly, therefore, of the nature of this disease, it is not enough for any one to form hasty conclusions from the peculiar data which may be presented to him, or the circumstances under which he is placed for a short time; for, however satisfactory to appearance such conclusions may be, they are "open to many sources of fallacy which it is impossible to guard against so thoroughly as to avoid the risk of error."* Every page in the history of medicine affords evidence in support of this statement, as well to theory in general as with regard to fever; and, bearing this in mind, we may now be prepared to examine such of the views of the epidemic fever of last year as either possess novelty or are of practical importance.

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[*] Dr Mackintosh's Principles of Pathology, Vol. i. p. 150.

"Men," it has been said, "fall in love with their own opinions, and grow impassioned about their own doctrines;"† but here I would disclaim both pretension to originality of theory and supposition of absolute rectitude on every point in which I may differ from others; and, if excuse or apology be necessary for reverting to the subject of this essay, I can only say that the following *Remarks* were, in some measure, forced upon my notice, and their correctness has been confirmed by personal observation while conducting the treatment of nearly 1300 cases. True, any conclusion deduced from the great majority of these cases, occurring, as they did, amongst the lowest of our population, would be utterly unworthy of credit were it founded on the reports of the patients themselves; but for this reason I have altogether avoided making any allusion to such phenomena in the history of the epidemic fever as were not observed by myself, or sufficiently authenticated otherwise. I may also remind such as have seen the disease under different and more favourable circumstances, and may consider my opinions as erroneous, that I am perfectly aware of the numerous disadvantages against which I have had to contend, so far as poverty, filth, overcrowding, &c. could affect my patients, and, on good cause shewn, am open to conviction; but, at the same time, I believe that the district surgeon who carefully investigates his cases, is not inferior to any other in his means of information.

The symptoms and variety of complications which characterized the epidemic fever of last year have afforded much room for discussion among physicians; and the question as to the *nature*, *origin*, and *causes* of the disease is still undecided. It has been spoken of as "yellow fever," "bilious fever," "remittent fever," "rheumatic fever," and so on. But has there been, hitherto, sufficient evidence adduced to show what was really the primary affection, and what were those secondary to it?—whether the jaundice, vomiting of bile, severe pains in the joints, and great tendency to relapse, have not been merely accidental circumstances, and not at all essential elements of the fever? The phenomena present during the febrile attack early in the season were certainly very different from what were seen in the latter weeks of spring and the months of summer and autumn; and, as the disease became more rare, it was found to resume its original character. Dr Mackenzie, on the authority of Dr Kennedy, states that cases of this fever were met with in the month of September 1842,* but the first well-marked case which I saw was in the beginning of January last year, and at that time I considered it the simple continued fever of this country. It occurred in my district in a house crowded to excess with lodgers; was supposed by the patient to be the effects of cold; slight bronchitis accompanied it, and the symptoms generally were those described by me † as constituting the primary form of the epidemic fever. The crisis took place on the tenth day, and the recovery was slow, but without any relapse. In the course of a week three other cases were seen, resembling the first in almost every respect, two of which were in families who had had typhous fever recently amongst them, but my patients had not been previously affected, nor was the disease of the same type as typhus. A few more cases were seen, all appearing in only one individual in a family, until the 25th of the month, when the healthy in the neighbourhood of the sick were the parties next affected, and the symptoms of the fever began to present some variety. In two families, the one in Goosedubs and the other in the Old Wynd, every member became affected in succession, and from them the infection could be traced to other families in the district. Hitherto also the type was mild, and bronchitis was the only occasional secondary complication; but in these families, while the early symptoms were of the usual character, the progress and termination of the disease were different. Of seventeen patients, thirteen had distinctly marked crises at periods from the seventh to the eleventh day; one recovered gradually without experiencing any crisis; and three, who slept in the same beds with the others, had their bodies covered with the *measly eruption of typhus*, and of these two died in the Fever Hospital. On the 21st February, I saw the first case of jaundice complicated with this fever; on the 8th March, the first case of diarrhœa; on the 14th April, the first case of excessive vomiting; and on the 19th of the same month, the first case in which numerous large petechiæ appeared. The bronchitic affection about this time became rare, while the cases with yellowness of skin, vomiting, and petechiæ increased somewhat rapidly, and continued to be frequently met with until the close of the summer months, when diarrhœa, often accompanied with vomiting, and sometimes with petechiæ, was by far the most common complication. During the whole of the epidemic season relapses very generally followed the first attack, after some days of convalescence, but almost universally so from May till October, by which time the fever had begun to decline, and to resemble more nearly what it was in the early months of the year.

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* Dr Christison in Library of Medicine, Vol. i. p. 154.

† Church and State Gazette, 24th November 1843.

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* London Medical Gazette, Vol. i. for 1843–44, p. 225.

† Edinburgh Medical and Surgical Journal, Vol. lxi. p. 68.

To investigate with as much accuracy as possible the true nature of this epidemic fever, it might be proper to describe fully the symptoms, and the order in which they appeared; but believing that every member of this society is already familiar with these, such a description has been considered unnecessary. To distinguish, however, the essential from the adventitious—the causes from the effects—in a disease presenting such a variety, is not always quite easy. " Our guide is invariableness of occurrence. . . . . . . If we can ascertain that a certain number of events invariably take place in every form and every degree of fever, these events will give us the particular phenomena which are common to all the varieties of the disease. If we can farther ascertain that these events invariably concur in a certain order, we shall have discovered what events bear to each other the relation of cause and effect."* If we apply this method of inquiry, then, to the particular fever at present under our consideration, we shall find that the disease was ushered in by certain symptoms which were never absent, and which, becoming aggravated in a few hours or days, constituted a simple fever, and that this fever in about a fifth of the whole cases—I speak of the disease only as I saw it—ran a continued course, free from all complications. We shall farther find that, in the remaining four-fifths of the cases, some were complicated with vomiting, some with jaundice, some with diarrhœa, and so on; and that in many of them vomiting, jaundice, and diarrhœa were present together, or vomiting and diarrhœa, or diarrhœa and petechiæ; in short, that to the primary constant symptoms were added a variety of affections, either singly or combined, in the majority of the cases, which gave rise to numerous modifications in the character and form of the fever. Nor is this all; for however much these complications varied the form of the fever, its course seemed little, if at all affected by them, and the tendency to relapse was not increased. Let me not be misunderstood. I have already stated that relapses occurred most frequently in the months from May till October, at which time jaundice, &c. prevailed to the greatest extent, but during the same months many cases of simple fever were seen, relapsing as the others did; and in November and December, when yellowness of skin never accompanied the fever, and bilious vomiting was rare, relapses were by no means uncommon, and, as often as they did occur, were not less severe than formerly. These are facts which bear intimately on every question regarding the nature of the epidemic fever; and reference will be made repeatedly to them in the course of these remarks. To proceed, then.

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* Dr S. Smith's Treatise on Fever, p. 38.

Dr Arrott, physician to the Dundee Infirmary, in concluding a "Letter" on the present epidemic fever, says, "the similarity of the symptoms during life, and of the morbid appearances observed after death, so nearly agree with the description of the yellow fever of the West Indies, and with the minute accounts of the Gibraltar epidemic of 1828, given by Louis, as to leave little doubt on my mind, that the only difference between these diseases and the Dundee epidemic, if difference there be, is a difference in degree and not in kind."* Now, as the Dundee epidemic seems essentially the same as that which prevailed so generally over Scotland last year, and as the opinion of Dr Arrott regarding its nature has been entertained by some, and denied by other physicians, how far can the history of the disease be urged in support of the views of either party? It is true that Dr Arrott would confine his observations "only to cases of yellow fever,"† these being, as I understand the expression, those cases of the epidemic fever in which jaundice prevailed; but it must be borne in mind that this feature of the disease was only of occasional occurrence, and altogether absent at the commencement and decline of the epidemic season,—a circumstance which never happens in true yellow fever, and is of itself a very cogent argument against the identity of the diseases in question. But "the term yellow fever ought to be entirely discarded; as yellowness being improperly viewed as a pathognomonic symptom of one kind of fever, all others in which it is a contingent phenomenon, although not more frequently met with in one than in another, have been confounded with that fever. . . . . Besides, yellowness is a very indefinite symptom; it not only is of various shades, but also arises from very different causes; it is livid or tawny in one case, brownish or mahogany-like in another, greenish in a third, orange-colour in a fourth, of a lemon tint in a fifth; and it is, moreover, either partially or generally diffused, or mixed with different colours, as with greenish, livid, purplish, violet, or reddish blotches. And it may proceed either from the passage of the colouring parts of the bile into the circulation, or from incipient dissolution of the blood, with loss of the vital tone of the capillaries."‡

Granting, however, that yellowness of skin was an essential character of our epidemic fever, do the whole symptoms bear such a similarity to the yellow fevers of hot countries, as to warrant us in concluding that the diseases are of the same nature, differing only "in degree and not in kind?" Judging from what I have seen of the one and read of the other, they appear to me to differ in kind and not in degree,—the only marked similarity being in the colour of the skin and frequent bilious vomiting, neither of which were uniform in their appearance, nor constituted the basis of the fever any more than the petechiæ, diarrhœa, bronchitis, &c. did. Black vomit is said to have been seen in fatal cases; and Dr Cormack thinks that "what is said by Blane, of the affection of the stomach and vomiting in the yellow fever of tropical climates, applies to our present yellow cases. 'In all stages,' says he, 'of this disease, it is the affection of the stomach that affords the most distinguishing and important symptoms. As it advances, an unconquerable irritability of this organ comes on. Whatever is swallowed, whether solid or fluid, of whatever quantity or quality, is immediately rejected by vomiting. An almost incessant retching takes place, even without any extraneous irritation, which commonly on the third day ends in what is called the black vomit, the most hopeless of all the symptoms attending it.'"* But my experience would lead me to consider these observations as in general not applicable even "to our present yellow cases," for the following reasons:—1*st*, Vomiting, of any kind, did not necessarily, nor always accompany yellowness of the skin; 2*d*, After vomiting had continued for two or three days, it generally disappeared, sometimes suddenly, but more often in a gradual manner; 3*d*, Black vomit, if ever true black vomit did occur, did so so seldom as to render its occurrence of no avail, as an argument of the identity of the two diseases; and 4*th*, I have never seen more than a dark-green, grumous-looking vomit, which, on being mixed with water, gave to this fluid a greenish tint, becoming fainter and fainter as the dilution was carried on, and only occasionally leaving a sediment,—showing that the greater portion of the egesta was really bile, while the remainder, no doubt, was blood thrown out by the capillaries of the stomach, as it was far more frequently from the nose, bowels, and other mucous surfaces. In fact, I am satisfied that I have never seen the black vomit of the West Indian fevers as I find it described by authors.

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* Scottish and North of England Medical Gazette, Vol. i. p. 133.

† Ibid. p. 129.

‡ Dr Copland's Dictionary of Practical Medicine, Vol. i. p. 987.

Farther, the morbid changes in some of the viscera found after death, in the bodies of those who died with yellowness of skin, have been considered as additional and conclusive proof of the identity of these fevers. On this point, however, I am hardly qualified to enter, as well from not having seen a sufficient number of inspections to enable me to judge from personal observation, as from the variety of published accounts of the anatomical characters, both of the yellow fever of the West Indies, and of the epidemic fever which has prevailed amongst us. But as far as my own experience enables me to form an opinion, I am inclined to believe that the diseased condition of all the structures in the latter disease bears as little similarity to the former, as the symptoms during life of the one bear to the other; and the more that I have seen of the epidemic fever, and read of the yellow fever of tropical climates, the more satisfied am I of the correctness of what has been stated, several months ago, by Dr Craigie, viz. " it is scarcely possible to believe that the distemper we see here can be compared in any of its essential characters with one so formidable and so fatal as yellow fever usually is."*

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* On the Natural History, Pathology, and Treatment of the Fever, &c. p. 109.

Whether the term " bilious fever" be applied to the epidemic fever either to denote its resemblance to the bilious or bilio-gastric fever of hot countries, or to indicate from the name the true nature of the disease, it appears to me to be equally inappropriate. The bilio-gastric fever of hot countries differs so little in several of its peculiar features from true yellow fever, that many physicians who have had ample opportunity of judging, have looked upon them as only one disease, occurring under certain modifications; and, of course, if we adopt this view, the arguments which have been already employed against the epidemic fever being yellow fever apply here also. But if we agree with those who consider the bilious fever as the ordinary, climate, or seasoning fever of the country,—a disease of " places and not persons;" never spreading by infection; occurring only in solitary cases, excepting where there is an accumulation of unseasoned strangers; and attacking the same individual not oftener than once, unless he have undergone intermediately a change of locality; by so doing, these very circumstances, together with the general symptoms, afford stronger evidence of our epidemic fever being altogether a different disease,—so different, indeed, that it is unnecessary to pursue this point farther. Nor does there appear upon what sufficient grounds the epidemic fever of last year can be viewed, as being in its nature purely of a bilious description. Vomiting of bile, yellowness of skin, and bilious diarrhœa were certainly frequent concomitants of the disease for a few months; but even when they prevailed most, pain in the joints, excessive perspirations, petechiæ, and various hemorrhages were as frequently observed. The mere frequency, however, of occurrence of a symptom does not of itself prove what the nature of a disease is in any instance, and how much less can it do so in the fever before us, which presented such a variety of symptoms as to render its nature unusually obscure? To establish the point that this was a bilious fever, it is necessary to show, not only that the symptoms generally were those of this character of fever, but also, that, individually and collectively in all cases, they could constitute no other type. But instead of this, we find that two or three of the more prominent symptoms of not nearly a half of the cases are, by a strange mode of reasoning, made the essential basis of the whole; to show the absurdity of which is by no means difficult. Thus, if it were just to draw any conclusion from one symptom, or series of symptoms, or secondary complications, we might consider the fever in females as depending on some uterine irregularity; for every pregnant female aborted in one or other of the attacks; every female, who had, previous to her illness, been menstruating regularly, menstruated, generally profusely, during the course of the disease; and, in many girls entering on womanhood, the catamenia then appeared for the first time, and have continued to flow naturally. Add to this, that epidemic puerperal fevers have resembled the late epidemic fever in some of the patients having "experienced attacks of ophthalmia," and "those who survived recovered very slowly, and were affected with wandering pains and paralytic numbness of the limbs, like those of chronic rheumatism. Some had critical abscesses in muscular parts of the body, which were a long time in coming to suppuration, and when broke discharged a sanious ichor."[*] Still no one supposes that the fever arose from a different cause in each sex; and the supposition, that a few symptoms of occasional occurrence were sufficient to explain the nature of the disease, could never have been heard of, had it been remembered that, "though the constitution of an epidemic gives to it a character, yet idiosyncrasy causes families and individuals to be affected differently from the generality of persons."[†]

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* Edinburgh Medical and Surgical Journal, Vol. lx. p. 416.

The excessive tendency to relapse in the epidemic fever has induced some physicians to consider it as resembling another of the fevers of hot countries. According to Dr M'Kenzie, "exanthematous typhus, that is to say, a continued fever, characterized, along with other symptoms, by an eruption over the body resembling measles, averaging a course of twenty-one days, and proving fatal to an extent of about 10 per cent., has been supplanted by a remittent fever," &c.;[‡] and one of his arguments, that this is the true character of the disease, seems to be derived from information, "that remittent fever in India is sometimes followed by comitis, and sloughing of the cornea."[§] Dr Craigie says, "When I saw the decided amendment about the fifth day, and the recurrence of the symptoms about the seventh, ninth, or eleventh day, I was disposed to regard the disease as a variety of remittent fever. This idea derived some confirmation from the mildness of all the symptoms about the third day. In many cases the mildness was very remarkable, in the headach being abated, the thirst being diminished, and the patient expressing himself free from complaint. The pulse was rarely much reduced."* Dr Cormack has also alluded to this remission on the third day, as having occurred in all the cases which he had an opportunity of attentively observing from the invasion onwards;† and I have seen patients again and again much relieved on the third or fourth day, but neither saw this remission so frequently as Drs Craigie and Cormack seem to have done, nor ascribed it to the fever being of a remittent type. My objections to this view of these authors are the following:—1*st*, The relapse which followed an attack of the epidemic was a return of the fever after some days of convalescence, irregular in its occurrence, and altogether distinct from the remissions in remittent fever, inasmuch as, in the former case, the patient was free generally of every febrile symptom, had his appetite for food restored, his strength daily increasing, and often had resumed his employment before he became affected again; whereas, in the latter case, the remissions and exacerbations are strikingly evinced, one paroxysm occurring in the twenty-four hours,—the remission being merely a temporary abatement of the fever. 2*dly*, The remission on the third day may justly be attributed to the effects of medicine given to the patient on the previous day, or, in hospitals, to his removal from amidst filth, impure air, &c. to a clean and well-ventilated ward. At all events, I have never seen this remission unless in such cases as I had reason to suppose might be affected by the treatment which I had prescribed; and I can believe that this effect would be more apparent, as well as more frequent, among hospital patients, since in these the exhibition of medicines was assisted by other means. 3*dly*, No argument can be founded on the occasional post-febrile ophthalmia, for we have seen that, besides the remittent fever of India, fevers in our country have been followed by affections of the eye. And, 4*thly*, From the morbid appearances in the abdomen alone, sufficient evidence is found to show that the diseases in question are essentially different,—the anatomical characters in the remittent fever bearing a greater similarity to yellow fever than to any other.

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[*] Dr Lee's Lectures on the Theory and Practice of Midwifery, pp. 497–498.

[†] Dr Cormack on the Natural History, Pathology, and Treatment of the Fever, &c. p. 106.

[‡] London Medical Gazette, Vol. i., for 1843–44, p. 225. [§] Ibid. p. 228.

I am indebted to Dr Lawrie for directing my attention to the description of a fever which prevailed epidemically in Arracan in 1825,—the only fever of tropical climate known to me which the late epidemic here can be said to resemble.‡ The similarity of the symptoms generally is certainly very striking; but similarity is not always identity, and beyond the symptoms these diseases are no longer similar; their origin evidently is different, they are different in their nature, and the treatment found most successful has been different also. "The lancet was had recourse to in almost every case . . . . . . Calomel and the compound powder of jalap were next exhibited as purgatives, and these were followed by pills, consisting of equal parts of compound extract of colocynth and blue pill, in number of from two to six, at intervals in the course of the day; and when any other medicine was required to keep the bowels open, salts and senna, or salts with emetic-tartar, were given . . . . Bark, wine, and opium were in general pernicious in this disease."* What would have been the effect of such treatment in the fever of last year? or what could show more forcibly the absolute difference in the nature of these diseases? Or, a more proper question, perhaps, would be, why does there exist such a desire to connect the epidemic fever with the fevers of hot countries? Surely the love of novelty which is so much displayed, both in the theory and practice of medicine, by some physicians of the present day, is hurtful when it leads to the conclusion, as it does here, that if cases with yellowness of the skin, &c. were really of the true nature of yellow fever, and so on of the others according to that they resembled most, we must have had several dissimilar and distinct fevers prevailing at the same time, generated the one by the other, and passing the one into the other. On the contrary, we know that "it is the nature of epidemic diseases, in their successive visitations, without leaving the marks of their species, to show altogether new lineaments, new times of accession, new periods of acmé, new phenomena in their termination, new forms of danger to be combated, and, fortunately, sometimes new courses of spontaneous relief;"† and knowing, also, that fevers bearing considerable resemblance, if not identity, to that which forms the subject of these remarks, have formerly prevailed epidemically in this country, why should we imagine that the epidemic fever of 1843 is different from these fevers, farther than can be accounted for by some epidemic influence?—a more rational and correct view, certainly, than the idea of its being recently imported.

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* Edinburgh Medical and Surgical Journal, Vol. lx. p. 417.

† On the Natural History, Pathology, and Treatment of the Fever, &c., p. 5.

‡ Transactions of the Medical and Physical Society of Calcutta, Vol. iii. pp. 105–127.

I need hardly allude to a supposition entertained at one time, that the epidemic fever was of a rheumatic disposition; for the parties who adopted this view, on becoming better acquainted with the disease, have seen cause to relinquish it. It evidently arose from the circumstance, that pain in the joints was an almost uniform character of the fever, and thus formed an essential feature of what constituted the disease; but the concurrence of many circumstances showed how little rheumatism had to do with the essential cause.

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* Transactions of the Medical and Physical Society of Calcutta, Vol. ii. pp. 119, 126.

† Scottish and North of England Medical Gazette, Vol. i. p. 236.

Following, as the epidemic fever did, in the track of typhous fever, prevailing in the same localities, and spreading in a similar manner, the opinion arose that these diseases were distinct only in some accidental occurrences; but more careful observation has shown that there are characters in the symptoms of the former so very unlike to what is seen in the latter, that few physicians now entertain any doubt of their being, to a certain extent, essentially different. These distinguishing peculiarities have already been noticed so fully by Drs Craigie, Alison, and Henderson, in the medical periodicals, and with which the members of this society must be acquainted, that it might appear like useless repetition were I to extend this essay by enumerating all the diversities of the two diseases. But some of these are so connected with this portion of my subject, that to pass them over in silence, merely because others had directed attention to them, would be paying a deference to authority which is quite uncalled for, as well as neglecting an important point in the train of argument. Besides, a surgeon in this city, who has certainly seen the epidemic fever as we saw it, although to a very limited extent, has stated in the Lancet* that his experience leads him to consider the diseases as essentially the same in their origin and nature; and, moreover, as his mode of proving their identity is by taking for granted, as a fact, what has long been a disputed question in medicine, what the majority of the profession in this country,—unless I am much mistaken,—are unwilling to admit, and what observation would convince me to be erroneous, a short summary may not be without advantage.

Mr Gibson, the writer in the Lancet to whom I allude, says, "It is generally admitted now, that an inflammatory state of the mucous membrane of the stomach, and the beginning of the small intestines, is the primary seat of typhus fever, and assuming that the fever in question is nothing more than a species of that fever, if not exactly the disease itself, with the exception of the intermission of six or eight days, the treatment," &c.—the inference to be drawn from which is that, were typhous fever not a gastro-enteritic affection, the epidemic fever, being evidently such in his opinion, would constitute a different disease. Of what the epidemic fever is, more shall be said hereafter; and without at all entering into the merits of the question, "whether an inflammatory state of the mucous membrane of the stomach, and the beginning of the small intestines, is the primary seat of typhus fever," I may be permitted to remark, that, so far as I am aware, this doctrine has never been very generally received by physicians in this country, and, that abroad many circumstances have occurred of late years, calculated to promote a change in the opinions of our neighbours on the subject of fever. The incidental discovery, as it was called, that the "epidemic meningitis," or "cerebro-spinitis," of Strasbourg, Avignon, and other towns, which, under the most active antiphlogistic treatment, carried off more than a half of those that were seized, was a non-inflammatory fever, requiring for treatment the use of saline aperients, cinchona, opium, &c., and the grievous want of success on the part of the medical officers of the French army in the late expedition to Africa, in treating the diseases that caused such ravages among the troops, have had no little share in opening the eyes of the profession throughout France to the errors of the system on which they have hitherto been acting. But there are other causes at work all tending to lead into favour the doctrine of a humoral pathology, and amongst these may be mentioned the great attention that is now paid to organic chemistry, arising, in no small degree, from the recent researches of Liebig, in connection with the functions of animal bodies. In short, "the doctrines of Solidism, so long almost universally received (by continental physicians), and seemingly so well established, are beginning to give way on every hand to those of a modified humoral pathology. Broussais and all his favourite notions have, for some time past, been at a great discount; the more sagacious of his disciples seem quite ready to yield up first one position and then another, and, while explaining away some of the extravagances of the system, to admit within the pale of their creed other morbid agencies besides those of irritation and inflammatory excitement. Even in the writings and lectures of M. Bouillaud, unquestionably one of the ablest of this party, we may observe symptoms of an uneasy consciousness that some of his long-cherished views have been carried rather too far; and M. Forget, the clever professor of the Strasbourg school, has lately begun to recognize the possibility of an altered condition of the fluids having something to do with many febrile disorders."*

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* Lancet, Vol. i. for 1843-44, p. 133.

Leaving this digression, we find that typhus fever, as it has been seen for several years past in this country, was unlike the epidemic fever in several respects, and so marked were the distinguishing peculiarities of these diseases as to lead at once to their detection. While the former, in the period of its duration, previous to convalescence, is seldom under fifteen days, and is longer in the majority of cases; is attended with a pulse for the first five days averaging "exactly 100 in the minute;"† in its course presents an "efflorescence of petechiæ,"‡ but better described as an "eruption of red spots"§ bearing "some resemblence to measles,"‖ and occurring in from 70 to 75 per cent. of the cases;¶

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* London Medical Gazette, Vol. i. for 1843-44, p. 216.

† Dr Henderson in Edinburgh Medical and Surgical Journal, Vol. lxi. p. 206.

‡ Drs Barker and Cheyne on Fever, Vol. i. p. 4.

§ Dr Craigie in Edinburgh Medical and Surgical Journal, Vol. lx. p. 411.

‖ Dr Christison in Library of Medicine, Vol. i. p. 141.

¶ Dr Davidson's Thackery Prize Essay, p. 22.

is prone to violent delirium, or other affections of the brain; seldom relapses during convalescence; generally attacks individuals only once in their lives; has an average mortality of about 10 per cent. &c.;—the latter is shorter in its attacks; has a more rapid pulse; presents no characteristic eruption; is seldom accompanied with delirium; re-attacks individuals frequently oftener than once; relapses in the great majority of cases during convalescence; and the mortality is about 4, or 4½ per cent. Moreover, the prevalence of the epidemic during part of the spring, summer, and autumn months, the presence of certain secondary affections, which generally complicated the disease, the character of these secondary affections, &c., might all be urged in proof of dissimilarity to typhus fever, inasmuch as this form of fever is found to prevail most in the winter season, is seldom attended with secondary complications, and these, when present, are always less or more dangerous. The very young and the aged, also, are much less frequently the subjects of the latter disease than those of middle age, whereas, no period of life presented any difference of susceptibility in the parties affected with the epidemic fever.

Such are some of those peculiarities which are calculated to show the dissimilarity of the epidemic and typhus fevers; at the same time, however, it must be admitted, that in certain of their characters they bore resemblance to each other. Thus, both diseases have prevailed most extensively in the filthy, ill-ventilated, and over-crowded localities of large towns, and amongst the poorest of the population; their modes of propagation, or communicability from the sick to the healthy, differ not; a few of their essential symptoms are the same; and "the fatal cases (in the epidemic fever) were, as usual in (typhus) fever, chiefly in old persons, or where there was obvious complication;"[*] in short, they present no evidence whatever of being essentially different, more than they individually differ from the fevers known as synocha and synochus, and I believe them to be "varieties of one fundamental disease, originating in the same causes, and constituted merely by differences in those obscure co-operating influences which are alluded to when we speak of epidemic constitution."[†] I find myself led to this conclusion both from what I have read of the continued fevers of this country, and from personal experience in treating several hundreds of cases among the poor; and, besides, viewing continued fevers "as one great malady never differing in nature, but in every two cases differing in intensity, and giving rise, by the differences, to various forms of disease,"[‡] is in accordance with the opinions entertained by some of the highest authorities in the medical profession. True, in some "particulars, *e. g.* the frequency of jaundice, and the almost uniform severity of rheumatic pains, this epidemic is certainly different from any that we have seen,"[*] still, as has been previously noticed, it bears such similarity as almost to prove its identity with other epidemics which have prevailed to a less or greater extent at various periods throughout the United Kingdom, and which have never been considered otherwise than continued fevers ; and it has been shown to be distinct from fevers of a remittent character.

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[*] Dr Alison in Scottish and North of England Medical Gazette, Vol. i. p. 2.

[†] Dr Christison in Library of Medicine, Vol. i. p. 126.

[‡] Dr S. Smith's Treatise on Fever, p. 73.

Another point of similarity between typhus and the epidemic fever, which seems conclusive to my mind either that the measly eruption, which is said to be characteristic of the former disease, is not so, or that the latter, under certain circumstances, passes into the former, and *vice versa*, or that both arise from the same causes, and owe their diversities and peculiarities to some unascertained agent, is found in the fact that this measly eruption has not been altogether absent during the epidemic season. I have found patients lying beside others affected with the epidemic fever covered with the eruption of typhous fever, and, after making the fullest inquiry, I am satisfied that those patients had not been exposed to the infection of this form of fever. Dr Cormack has also seen this disputed symptom occasionally among his patients, these "apparently getting the disease from a contagious poison evolved from, or generated by, persons congregated together, affected only with the prevailing form of the fever ;"[†] and I subjoin the following particulars of some cases, "carefully investigated," which occurred in the course of my practice last year, as possessing some interest from their being at variance with the experience of Dr Henderson and others, together with the proof derived from them, that though "the diseases in question are, undoubtedly, very different in form, yet not more different than the mild character which we often see epidemic measles almost universally assume, from that fearful pestilence, the offspring of the same poison, according to all orthodox belief in medicine, which has been more than once known in Edinburgh, even in the memory of medical men still amongst us."[‡]

Mrs M'Gill, residing in the Highland Close, 6, Goosedubs, became my patient on the 25th January 1843, having the usual symptoms of fever as observable on the third day of attack. As she lay in a lodging house, her landlady urged me to send her to the Fever Hospital, lest other lodgers should become affected; but as the case after a day or two did not present the true typhous character, and as it was wished to confine the admission into the hospital to typhous fever alone, I refused to comply with her request, and the patient was allowed to remain. On the tenth day of her illness a crisis was experienced, marked by profuse perspiration, and followed by relief of all the symptoms; but on the fifth day of convalescence the fever returned with nearly all its former violence,—the relapse being ascribed at the time to carelessness in her diet, or other irregularities. Previous to her relapse, however, her husband, who had slept beside her all along, became affected with fever also; but when I saw him on the fifth day of attack, his skin was mottled over with a measly eruption, and the symptoms generally were such as to satisfy me that this was typhus; and, at his own request, I sent him to the Fever Hospital, where he died. Six other persons, one after the other, were affected with the epidemic fever in this house, of whom three relapsed, and two of the cases were complicated with pretty severe bronchitis, but in none was there any exanthematous or other eruption. In another house, 114, Old Wynd, on the 14th February, I found four persons ill of fever, two of them presenting the general symptoms of typhus, while the other two were well-marked cases of the epidemic fever. All were sent to the Royal Infirmary or Fever Hospital, where one of the former died; and on the return of the two latter to their home, both relapsed; and during their convalescence, and afterwards, five other cases of epidemic fever occurred in succession, in the same house. On the 20th February, in visiting in another lodging-house, 66, Old Wynd, I saw two girls confined to bed, the one severely ill and covered with the measly eruption of typhus fever, but the symptoms of the other were altogether so different, that, although lying in the same bed with the former, I considered her case as distinctly of another character. Next day I found a third girl affected; and, in two days afterwards, at the earnest request of her landlady, I granted a line of admission to the Fever Hospital to the first of these, and she was admitted as a "proper patient." The other two were treated at home; one of them, in addition to her other symptoms, presented the first case of jaundice I had seen, and both relapsed. Every person in this house, in the course of a few weeks, had epidemic fever. In a house in Guildry Court, in the course of the autumn, I attended a patient who had epidemic fever accompanied with yellowness of skin, bilious vomiting, &c., and the next person affected was his bed-fellow, in whom the disease resembled typhus fever, accompanied with the measly eruption. The same thing occurred in two families in Stockwell Street; from one of which two patients were sent to the Royal Infirmary, and the cases acknowledged to be typhous fever by the physician under whose charge they were placed; while the first affected, those treated at home by me, had the peculiar characters of the epidemic fever distinctly developed, even to the relapse.

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[*] Dr Alison in Scottish and North of England Medical Gazette, Vol. i. p. 2.

[†] On the Natural History, Pathology, and Treatment of the Fever, &c., p. 106.

[‡] Scottish and North of England Medical Gazette, Vol. i. p. 240.

In the preceding remarks, suggested by the names which have been given to the epidemic fever, my views regarding its true nature may, to some extent, be traced. But, to be more explicit, I believe the disease to be a mere variety of the continued fevers of this country, accompanied, in a great majority of the cases, with several complications, these being not essential conditions of the fever, yet giving to it its distinctive character. In other words, I consider those symptoms, or series of symptoms,—apart from bronchitis, vomiting, jaundice, diarrhœa, &c.,—which have been found to prevail in every case, and which alone were always present in the early, and not unfrequently in the latter months of last year, as in essence constituting the fever; while the others, being of irregular occurrence, were nothing more than the accidents of epidemic influence. Indeed, if invariableness is to be our guide in directing us to distinguish the real from the apparent,—the essential from the adventitious, I cannot see what other conclusion can be deduced from the history of the epidemic; for, as has been already stated, from its first appearance onward throughout the year, cases free from all complications have been observed to be of the same duration, and to have periods of convalescence and relapse alike to those in which the secondary affections were the most prominent features. How often have I seen a family, of whom several members were affected, one with yellowness of skin, without vomiting, another with vomiting, a third with vomiting and diarrhœa, a fourth with some inflammatory action in the chest, a fifth in whom no local disorder existed, and so on; nevertheless, in all these there could be perceived certain characteristic symptoms which issued in the disease, and accompanied it during its progress. Farther, the patient with simple fever communicated to another a form differing from his own, in as far as the presence of any non-essential symptom, by becoming the prevailing symptom, could form a difference,—a difference in degree and not in kind; and a third person, who received the infection from the second, would be found to have some other symptom more urgent than the rest, which gave to the fever, in his case, often the appearance of a distinct disease. In what manner, then, are we to explain these phenomena? Either we must adopt the view just stated, and which, I believe, the whole history of the epidemic proves to be correct; or some one or other of what I have considered the secondary affections must be taken as the type or basis of the fever,—a view which, with respect to several of these, has already been shown to be erroneous.

The epidemic fever presents to us a subject of as much interest in investigating its causes, as can be derived from any inquiry regarding its nature, and, did my limits permit, would have devoted to its consideration an equal share of attention. I find, however, that already I am encroaching greatly on the time of the society, and should be drawing these remarks to a close. In what follows, therefore, I shall be as concise as possible.

Of the primordial source of the epidemic fever little need be said; for that, like the source from which "other contagious fevers originated, is involved in absolute obscurity;"* and, whatever some may say of a new fever, capable of communication from the sick to the healthy, being generated by over-crowding, filth, impure air, poverty, and such like, facts seem still to be wanting to establish the theory, or to give it even a semblance of plausibility. No doubt, cases have occurred of fevers arising, so far as could be seen, from the causes alluded to; but have these spread again by infection beyond the limits of their generation, or where the source of their origin could not influence their propagation? or, could it be possible that the germs of a fever already existed, and only required the operation of an exciting cause to manifest the disease? Nor is the difficulty of the question either lessened or explained by several writers, who, finding themselves unable in any way to account for the epidemic appearance of certain diseases by means cognizable to the senses, "have had recourse to various supposititious agents, of the nature of which they are entirely ignorant, and even the existence of which they have not been able to demonstrate."† We know that the epidemic fevers of this country, as well as those which prevail elsewhere, are generated by certain poisons, the deleterious effects of which have an inherent tendency to pass off, after having caused a definite train of symptoms; but beyond this all is vague theory; and I quite agree with Dr Cormack that no sufficient evidence has yet been adduced to determine satisfactorily "whether the morbid poison which causes the present disease be or be not that which gives rise to the true exanthematous typhus merely modified by the operation of other poisons, terrestrial, atmospheric, or imported."‡

But there can be little doubt that poverty and its consequents so act upon the human frame as to render it obnoxious to the influence of disease; and the late epidemic fever affords another example of the truth of this statement. Nor can ought else be looked for; and our only astonishment is, not that the poor become pre-eminently the subjects of fever, but that they can exist under the privations and amidst the discomforts which they experience. In such places as my district,—and it is in such places and among such a population that fevers prevail epidemically,—the clothing of most of the inhabitants is little more than sufficient to cover the nakedness of their bodies; their diet is generally of the coarsest description, and not over abundant; their houses are small, filthy, over-crowded, and constructed without regard to ventilation; and altogether their habits, caused too often by their mode of livelihood, are irregular and destructive to health. This, of course, is not the character of the poor everywhere,—far from it; but I have had ample means, for many years, of assuring myself that it applies pretty extensively to paupers in large towns; and farther, I believe that poverty is not only very frequently the result of profligacy and misconduct, but is kept increasing by an indulgence in many vices which tend to hurt the constitution, such as the immoderate use of ardent spirits, and the almost universal practice in that unmeaning and disgusting habit, tobacco smoking.

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* Dr Davidson's Thackery Prize Essay, p. 2.

† Dr Copland's Dictionary of Practical Medicine, Vol. i. p. 769.

‡ On the Natural History, Pathology, and Treatment of the Fever, &c., p. 115.

From the fact that the epidemic fever of last year made its appearance, and began to spread in those localities where poverty and wretchedness of every description most abound; and that, during the whole season of its prevalence, the pauper population were almost its only victims; there is presumptive proof that the propagation of the disease from one person to another, among the poor, depended in some measure on such circumstances as deficient clothing, over-crowding, impure air, want of cleanliness, &c. together with scarcity of food; but in what manner these operated in accomplishing the end has not been well explained. That they were not the source of the fever I have already stated, and believe to be established by abundant facts, if we may judge from analogy and the history of other epidemics. How, then, is their agency brought into action? Do they merely act as predisposing causes, or, in connection with the peculiar influence of epidemic constitution, do they become the exciting causes? It has been ascertained that disease "in certain situations and circumstances, as in low filthy places; in crowded and ill-ventilated streets and houses; in stagnant, moist, impure, or other states of the air; from the confinement of a number of sick in small space; want of cleanliness, or bad habit of body; in states of physical and mental depression, &c.; may assume a more malignant character, and emit an effluvium which will become either a superadded cause, concurrent with apparent antecedent causes, in diffusing the malady, or a principal agent of infection, or possibly even of contagion, to which the others are entirely subsidiary;"* and if such be admitted of disease in general, the principle applies equally to the case in question. The early progress of the fever, formerly mentioned, is perfectly in accordance with such a view,—nay, rather, is illustrative of its truth; and I doubt not that elsewhere like phenomena presented where the like causes were in operation. Thus, the enquirer is led to trace, step by step, the epidemic from

* Dr Copland's Dictionary of Practical Medicine, Vol. i. p. 773.

its first occasional, and as it were sporadic appearance, until by its more frequent occurrence in those localities which are known to be favourable to the spread of disease, a new cause was brought into action, whereby its communicability from the sick to the healthy was greatly increased, and to which, viz. infection, much of the rapidity of its progress afterwards was pretty generally ascribed.

Of the infectious nature of the epidemic fever, however, as is usual with whatever relates to medicine, there is not unanimity of opinion among physicians; but, after what I have advanced capable of being made to bear on this point, more can hardly be said without discussing fully the whole question of infection; and it would require no little power of reasoning, even then, to convert some of those who are opposed to the view which I, in common with the great majority of the profession, entertain regarding the diffusion of this fever. What could be more convincing than to see the disease attacking every individual in a family, the one after the other, and next appearing in another family, some member of which had been in the habit of visiting or attending on the diseases, and in this manner spreading throughout a number of families,—the *foci* becoming more numerous as the epidemic season continued. As observed by Dr Christison, "it may often be difficult to trace out these facts with accuracy, partly on account of the great length of time the infection often lies latent before the disease breaks forth, partly by reason of the apathy of the order of society where chiefly it prevails, and their natural forgetfulness of what is passing even in their immediate vicinity. But at the beginning of an epidemic, before the question is complicated by the establishment of secondary foci for the disease, it may be always ascertained, with proper pains, to spread gradually to the neighbourhood of the sick in the first instance."* And that such was the manner in which the late epidemic fever spread has already been noticed, and must be known to all who had the opportunity of observing its progress in the early months of last year. Circumstances, which it is unnecessary I should repeat here, led me at the commencement of the season to note carefully the history of my cases; and having charge of one of the districts in which the fever first was observed to increase, I enjoyed advantages of being enabled to trace case after case with more ease and more satisfaction than could be done very shortly afterwards. In a word, I feel inclined to believe that the "non-contagionists" have seen little of this epidemic in the dwellings of the poor man, amid cold, and hunger, and nakedness, and misery that cannot be named; for it is only in such places that frequent and close communication is kept up between the sick and the healthy, and where the disease is found "creeping from one individual to another in a family, then from family to family according to their proximity, relationship, or general intercourse, and at length to the surrounding population promiscuously, with the exception of the better ranks."*

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* Library of Medicine, Vol. i. p. 155.

Glasgow, 18*th February* 1844.

Art. V.—*Facts and Observations on the Sanatory State of Glasgow during the last year; with Statistical Tables of the late Epidemic, showing the connection existing between Poverty, Disease, and Crime.* By Robert Perry, M. D., President of the Faculty of Physicians and Surgeons, Senior Physician to the Glasgow Royal Infirmary, &c.

No apology can be necessary for bringing before the public such facts connected with the progress of the present epidemic as have come under the observation of one connected with a large public hospital, where ample opportunities are presented of tracing the influences which favour its spreading, or increase its virulence.

From the month of December 1842, when sporadic cases of this epidemic first began to show themselves in this quarter, there was almost a constant prevalence of cold easterly winds. The poorer classes of society had for the last two years suffered great privations in the comforts of life, so that their constitutions might be less able to resist external influences of an injurious tendency. During the months of January and February, catarrhal affections, accompanied with derangements of the biliary organs, were more than usually prevalent. Among the more delicate of the female patients in the hospital, it was observed that many were attacked, on the least exposure to a stream of cold air, by rigors followed by an exacerbation of fever, which at the time was attributed to the nurses having thrown open the windows for the purpose of ventilating the wards. These cases occurred more frequently in ward No. 9, which is colder and more exposed than the other wards, the windows facing the north-east.

As the season advanced these cases became more frequent, and more constantly accompanied with severe headach, vomiting of bilious matter, and yellowness of the eyes and skin, and, before the real nature of the epidemic was understood, several cases were admitted into the hospital as cases of jaundice. Indeed, the first well-marked case of the disease which came under my care was admitted in December 1842, as a case of jaundice; which, on looking over the reports taken of the case in the hospital journal, I find the symptoms are all accurately described;—"the rigor, the severe headach, the bilious vomiting, pain of epigastrium, increased on pressure, urgent thirst, quick and firm pulse, all abating; and then on the eighth day the pulse falling to 80 but the patient complaining of rheumatic pains of the joints. On the fourteenth day another rigor followed by heat of skin, increase of thirst, and on the third day going off by a copious perspiration." Many of the cases admitted were of a much milder character and less regular in their symptoms than the one noticed, going through all the stages in a modified degree; and such cases at that period generally received the name of febriculæ, and by the poor among which it was prevalent, influenza. In many of the cases coming from the poorer localities, livid petechiæ were present; and in the young, epistaxis was not uncommon. Diarrhœa was also frequent, the stools occasionally tinged with blood. As the season advanced the disease spread, and the symptoms became more severe; on the pains abating, the feet and legs often became œdematous, and the patient so weak that several weeks elapsed before he or she were able to resume their usual employment. The average period after the relapse was fully three weeks before the patient was fit to be dismissed and follow his usual employment, and often two months where they had not the means of obtaining nourishing diet and comfortable clothing, which among the poor was generally beyond their reach. In a few cases delirium was present, which seldom lasted above four days, and differed much from the delirium of typhus, though in some cases in old people permanent stupor was observed, ending fatally. The having previously passed through typhus was no security against attacks of this disease; in several instances the persons recovering from the one were seized with the other. As the epidemic spread cases of typhus became more rare. Another circumstance, which was also a very constant concomitant of the disease in females, was the occurrence of the menstrual discharge, although not at the stated period; and, if pregnant, abortion took place. Among the sequelæ of the disease, local inflammations, as pneumonia, diarrhœa, or dysentery, and occasional amaurosis, occurred. A second attack of the disease after a lapse of a few weeks sometimes took place. It was not uncommon at one period of the epidemic for herpetic eruptions to come out around the mouth and nose about the period of the relapse. Another circumstance which I have not seen anywhere noticed, was the increase of appetite, or craving for food, which the patient felt a day or two before the occurrence of the relapse. Those patients who had formerly passed through typhus described their sufferings from this present disease as being far more severe; arising, I have no doubt, from their being less conscious during the former complaint, and the memory being generally impaired. The circumstance of this disease attacking those who had formerly passed through eruptive typhus, and being followed by typhus when the patient was exposed to the infection of the latter, is sufficient to show that they are totally different diseases both in their character and causes: as different as small-pox and scarlatina, or typhus and cholera.

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* Dr Christison in Library of Medicine, Vol. i. p. 155.

Indeed the present epidemic more closely resembles cholera in its mode and progress throughout the country than any other epidemic I have witnessed. It made its appearance like the cholera in sporadic cases, and in various districts of the country about the same time. It was spreading in Greenock, Paisley, Kilmarnock, and other parts in the west of Scotland, while at the same time it was spreading in other quarters of the east, as in Edinburgh, Dundee, and I believe in Liverpool. It attacked in preference and most severely, the same class of persons as cholera did, viz. the broken down and destitute, and was most severe on those whose constitutions were naturally weak, or had been reduced by want, by age, by disease, or intemperance. In both diseases the blood and biliary systems were severely affected, as evidenced in the present epidemic by the livid petechiæ, the bilious vomiting, and by the collapsed or sunk state into which many of those severely affected fell. There are other coincidences which might be mentioned as being occasionally met with,—as the weak or altered voice, the sunk or hollow appearance of the eye, with frequently an injected state of the conjunctiva of one eye, or part of the eye. The two diseases differed, however, much in their symptoms, and materially in one important part, in their ratio or law of mortality; the one cutting off nearly a half of all who were attacked whatever was the treatment; the other only from three to four per cent., with or without any medical treatment, although in many cases the patients appeared to receive much benefit from the treatment in alleviating the symptoms.

It has been stated that a fever of a similar character to the present prevailed in this country in the year 1818; I have been not an inattentive observer for upwards of thirty years, and have never witnessed a similar disease in this city.

The question has been frequently asked, What is the cause of the present epidemic? Respecting the immediate cause of any epidemic disease with which a community may be visited, very little satisfactory information has hitherto been obtained; so that to give a definite answer in our present state of knowledge is beyond our power. Attempts have indeed been made, and those who have the least experience generally speak with most certainty on the subject. At present it is the fashion to ascribe every epidemic, whether malignant fever, cholera, dysentery, or influenza, to malaria arising from decaying animal or vegetable substances, owing to the want of sewers for carrying off such substances, and the scanty supply of water. There are perhaps few places better supplied with water than Glasgow; and I have observed on more than one occasion during the prevalence of malignant fever, that its progress was equally rapid and violent during a period of intense frost, when everything has been covered with snow, and the whole liquid substances in the streets firmly bound up for weeks together, without the possibility of any putrefaction going on. This was particularly the case in 1837, when the frost continued very intense for upwards of six weeks, when the number of fever cases was greatest. The same thing was observed at Moscow during the prevalence of cholera. It appears that some epidemics are spread solely by means of infection; these are specific poisons, generated in the bodies of those who are undergoing the disease, and spread either by contact, or by the emanations from the bodies of those affected by the specific disease. This is the case with all that class of infectious fevers called exanthematæ; as small-pox, measles, scarlatina, and typhus. There are other epidemics which seem to depend on some peculiar state of the atmosphere predisposing the bodies of those whose constitutions are weak to suffer under their influence, of which the present epidemic affords a good example. This brings us to the more practical part of the question, viz. the causes which predispose persons to be affected by any prevailing epidemic: and in this inquiry very little difficulty presents itself, there are so many facts which attest that it is the poor and indigent part of the population who furnish the earliest and by far the greatest amount of victims.

In bringing the state and sufferings of the poor before those whose duty it is to watch over their welfare, it is not my intention to propose any theoretic plan of relief, but to state such facts as have come under my own observation and the observation of those who have had the very best opportunities of witnessing the miserable circumstances in which a great mass of our poor population are situated, and on whose testimony (given, as it has been, without knowing what use was to be made of it) every reliance may be placed; I mean the district surgeons. Most of those gentlemen at my request have kindly furnished me with excellent reports of the state of the poor in the districts they attend. In a few cases I have failed; whether from apathy in obtaining any statement I am not prepared to say. I am certain these reports will be read with a deep and painful interest by all those who are alive to the welfare of their fellow-creatures, and ought to call forth the immediate exertions of all who have any influence in remedying such a state of things.

In looking over these reports, one striking feature in the circumstances of the thousands who have suffered most from the present epidemic presents itself; that is, the over-crowded state of their houses, families of six, eight, and ten individuals crowded into one small apartment, without a bed to lie upon, if we except, perhaps, a quantity of long-used straw or filthy rags, emitting a stench of human impurity so offensive, that to one unaccustomed to it is wholly insupportable, but to which the wretched inmates from habit appeared totally insensible: those small apartments, being often let by the week, are filthy in the extreme. Being tenants at will, (like tenant at will farmers,) they take no pains to clean or improve their houses, knowing that if they present a greater appearance of comfort, a higher rent will be sought, and if offered by another accepted, and if falling behind in their rent for a week will be ejected. They will not then be at the trouble, far less the expense of improving an uncertain possession, and thus acquire habits of sloth and filthiness.

Some have imagined that by destroying these wretched abodes, by pulling them down and building better houses and wider streets, they would remedy the evil. This is taking a very limited view of the distress, and betrays a lamentable ignorance of the cause; for the poor, not having the means to pay for a better than they at present possess, must still continue to huddle together in dwellings scarcely fit for pigs. As to the causes which have led to this state of destitution and wretchedness among the poor, men will differ according to their prejudices and interests, one class ascribing it to restrictions on trade, another to the improvements in machinery, a third to the want of moral training and habits of intemperance, and a fourth to the amount of taxation, often mistaking effects for causes.

That there is a superabundance of labour in the market will readily be admitted, and should the population continue to increase as it has done, at the rate of a thousand per day, and our markets not extend in a similar ratio, the evil must necessarily rapidly increase.

It is in large cities where the greatest amount of misery is to be found, and the darkest picture of the effects of man's cupidity is exhibited. There the human species, more vicious than their inferiors, are seen preying upon each other, each in his own way, from the monopolist down to the common thief,—the former with the sanction of the law, the latter by stealth and violence.

This over-crowding in large cities, and consequent accumulation of poverty and vice, is increased by the very improvements which add to the comforts of those who are superior to want. Among the improvements which of late years have led to this increase, railways may be mentioned. It is now found that all the small towns and villages near the tract of the railways are rapidly falling into decay. The facilities afforded to those who have money to spend, encourage them to visit and spend their money in the large cities or towns, where there is a terminus, so that those who were formerly able through their means to obtain a livelihood by their custom, are now forced to follow, and seek the means of subsistence as they best can, among the crowd who are struggling for existence.

In reflecting upon the immense amount of destitution and misery which exists, it is not possible to avoid coming to the conclusion that there must be something wrong in the state of society when the population of a country, acute in intellect and fertile in resources, is plunged into such a state of hopeless misery, a misery which is yearly and daily increasing in intensity, and extending wider among people formerly noted for their sobriety and industry. It is certain that ere long, the rich will be compelled in some shape or other to look to the support of the poor: let those, therefore, who have influence, consider well, if nothing be done soon, what the end of this state of things may be. As one means of relieving those who are able and industrious, could an extensive system of emigration to our own colonies not be adopted, which, by extending our markets, would give greater employment to those who remain?

Those individuals from the country villages whose resources are dried up by the means above noticed, are well adapted for colonists; and, besides benefiting themselves, would soon become the employers; instead of in the city mingling with their families, with those by whom they must almost necessarily be infected both by vice and disease.

In the appendix to the report of the district surgeons, it is stated that, " In 1841–42, 5296 cases received medical relief; this year, 19,058 cases; increase this year, 13,789 cases. Of this year's report, 12,967 were cases of a peculiarly distressing epidemic fever, and 736 cases of typhus fever. Of the former 12,967 cases, 12,397 were treated at their own homes, and 570 sent to the Royal Infirmary. Of those treated at home, 304 died, being in the ratio of 2.363 per 100 cases." The extent and progress of the epidemic, and the causes which have favoured this extension, will be best understood by giving a monthly return of the new cases of fever, from the different districts, made up from the weekly reports, accompanied with a map of the city divided into the districts, and marking the localities where the epidemic was most prevalent. The circumstances of the population will be learned from the reports of the district surgeons in the appendix.

To show the progress of the epidemic throughout Glasgow, I have laid down and numbered the different districts upon a map of the city, as they are attended by the district surgeons, and also those of the barony, marking with a darker shade those parts where the epidemic was most particularly prevalent, which shows that those places most densely inhabited by the poorest of the people have suffered most severely. The epidemic, having once got into a densely-crowded land or close, never ceased until it had visited every house, and in many of the houses every inmate.

The number of cases of the present epidemic reported to the directors of the town's hospital, as attended by the district surgeons during the last eight months, commencing May 1843, amounted in round numbers to 14,000, as seen by the table. Of those occurring in Calton, Bridgeton, and Anderston districts, I have got no accurate return, but it is certainly within the mark to estimate it at 7000; those in the Gorbals and Tradeston districts at 2000; those treated in the Infirmary during the same period at 3000; those who were attended by private practitioners, not in the poor roll, at 5500; and those treated in the barony parish, in the suburbs, without the parliamentary boundary, at 500; making a total of cases of epidemic fever in Glasgow and suburbs, during the last eight months, from the beginning of May till 30th December, 32,000, exhibiting a fearful amount of poverty and disease, as it affected the poorest and most destitute of the population. Great as it is, however, I have been assured by the various practitioners who have attended the poor, that the estimate is far too low.

Dr Garroway, in his excellent report of the Bridgeton district, estimates the number there at 9 per cent. of the whole population; and at that period, 3d of November, the epidemic had not completed its ravages.

The following table exhibits a monthly return, for eight months, of the new cases of fever, as they occurred in the different districts throughout the city, among the pauper population; the progress of the epidemic from one district to another; also the number sent from each district during the same period to the Infirmary;—showing how inefficient were the means employed for checking its progress by isolation.

TABLE I.

Monthly Number of Cases in each District.								
District.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1	50	152	266	289	326	236	124	173
2	13	54	193	304	168	124	96	86
3	35	71	323	317	206	125	60	92
4	132	222	139	243	142	85	66	73
5	15	32	53	81	71	33	41	38
6	15	30	30	56	28	48	32	35
7	13	17	69	130	167	113	108	111
8	0	19	73	141	109	87	66	49

Monthly Number of Cases in each District.

District.	May.	June	July.	Aug.	Sept.	Oct.	Nov.	Dec.
9	86	123	211	368	321	196	110	79
10	0	90	253	572	445	198	72	111
11	63	170	218	380	296	106	91	80
12	29	43	123	228	178	97	59	74
13	25	56	71	157	121	89	71	52
14	5	25	81	125	100	100	91	69
15	9	16	48	114	108	161	90	113
16	12	15	20	79	114	75	39	28
17	8	8	13	55	90	57	59	107
Totals.	510	1143	1284	3649	2990	1930	1275	1370
Number of Coffins given out by the Poor's House monthly.	121	148	162	217	256	193	146	105

Number sent to Infirmary monthly from each District.

District.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Population.
1	3	7	4	21	5	6	0	1	
2	5	2	4	56	8	1	0	3	
3	11	2	3	3	6	0	7	4	
4	16	2	2	1	0	0	0	0	
5	14	17	8	12	14	2	3	0	
6	6	10	5	11	1	0	2	1	
7	7	1	0	3	0	0	0	1	
8	0	1	2	1	2	0	0	1	
9	23	22	16	3	1	0	3	1	
10	0	0	4	2	0	2	0	2	
11	13	3	7	5	0	1	2	1	
12	5	6	5	14	2	6	4	2	
13	9	1	5	13	1	2	2	0	
14	0	15	17	21	3	5	4	2	
15	3	0	7	14	10	2	4	3	
16	10	1	1	2	0	0	0	1	
17	4	0	0	4	1	5	2	8	
Totals.	130	90	90	185	54	32	33	27	118000

Since the foregoing table was drawn up, an abstract of the Glasgow Mortality Bill has been published by Mr Watt, which shows that I have under-estimated both the number of the fever cases and the proportionate amount of mortality, as he shows an increase of the mortality over last year to the extent of 2.340, which is seven and a-half per cent., and this increase wholly confined to the poor.

The following table will show the amount of the population in the different localities, the estimated number of cases of the epidemic, and the per centage they bear to the population, according to the best data which could be procured.

TABLE II.

District.	Population.	Cases of epid. attd. by dist. surg.	Av. per cent.
City districts, . . .	118,000	14,000	12
Calton, Bridgeton, Camlachie, Wilton, Blythswood, and Anderston, . .	99,000	7,000	7
Gorbals and Tradeston, . .	48,000	2,900	4.5
Remainder of Barony parish in the suburbs, without the Parliamentary boundary, .	10,000	500	5
Cases treated by private practitioners, .	...	5,500	...
In the Infirmary, . . .	...	3,000	...
	275,000	32,000	11.63

The above table shows that of the whole population the epidemic has attacked eleven and a-half per cent.

By the kindness of Mr Strang, City Chamberlain, I have obtained an accurate statement, from the Government census of 1841, of the population of six of the densest city districts, where the fever was most prevalent; and the following table will exhibit the actual number of those attacked by the epidemic in these districts, also the average per cent. in reference to the population.

TABLE III.

District.	Population.	Cases of the epidemic.	Average per cent.
1	11,910	1616	13.56
2	7169	1038	14.47
3	5166	1229	23.79
4	5466	1102	20.16
11	5263	1404	26.65
12	4557	831	18.23

These are the districts within the city where epidemic disease is always most prevalent.

The returns from the Glasgow Royal Infirmary show a mortality from the fever of 5 per cent. The city district surgeons estimate theirs at 3 per cent., but from the table of the number of coffins given out by the town's hospital to the poor during the epidemic period, it is probable that there is some fallacy, as it is known all the poor on their roll do not apply for coffins; and that during the epidemic period there was a remarkable immunity from all other diseases. If we subtract 500 from 1346 it leaves 846, being more than the whole number of coffins given out during the whole of the previous year, giving a proportion of mortality to the epidemic in the city of about 6 per cent. during the eight months of its prevalence; but taking the whole at 5 per cent., the mortality will amount to 1500 as the result of the present epidemic during a period of eight months.

As comparatively few of the sufferers belong to the middle ranks, being, as already remarked, chiefly confined to the poor and labouring classes, a large proportion of them being adults, some idea will be formed of the amount of suffering among the poor, by considering that, on an average, they were unable to follow their usual employment, or gain anything for their support, for five weeks; and calculating their weekly earnings at the small sum of 4s. weekly, it exhibits a positive loss to the poor, in eight months, of not less than L. 32,000. Had this been the result of a strike among the workmen, the whole newspaper press would have been filled with lamentations; but being the result of causes over which the poor had no control, the press has been comparatively silent, and little has been done for their relief. When it is considered, besides, that Glasgow has not suffered alone, but that the epidemic has visited nearly every large town in this country, the effect ought to call forth some effectual means of relief, and the devising of measures for preventing the recurrence of a similar or even worse calamity, which, in the present circumstances of the country, may with certainty be predicted.

Credit has been taken for the saving supposed to have been effected to the community, by treating the poor in their own houses, in place of sending them to the Infirmary, where their keep and treatment would have cost the community about 15s. or 20s. each, whereas it has been assumed that it only cost the public a few hundred pounds. Against this doctrine I beg to enter my most solemn protest; and if those who hold this opinion believe the disease to have been contagious, I ask, if the public had possessed the means of isolating the first cases as they appeared, by sending them to an hospital, would the epidemic not have been checked? and in place of having 32,000 cases, with all their accompanying sufferings, it might have been limited to less than 5000?

It is true that, in the present state of matters, funds might not be forthcoming, nor will they, under the present system, until some serious epidemic, as typhus fever, has made such progress among the community as to commence its ravages among the upper classes, when fears for their own safety will compel them to take measures, when it is too late, to check the progress of the pestilence.

If I am asked what measures I would recommend, I answer, in the first place, let the authorities earnestly urge upon the legislature the necessity of a poor law, by which the whole property of the country shall be taxed, for the support of the poor in hospitals afflicted with epidemic fever. Let there be a board of health established in every large city and town, at the head of which there is placed a medical inspector, whose duty it will be to receive reports, visit, and inspect the different districts of the city. Whenever epidemic disease is reported, let him give in his report to the board of health, whose duty it shall be to cause the proper officers to remove the infected, and have the houses cleaned and purified, and, with the assistance of the police, keep a strict surveillance over all lodging houses. We have already part of the machinery constructed under the cholera act, when a board was appointed for the suppression of that pestilence. Such is a short outline of the plan which I think ought to be urged upon the Government by the authorities, and I am satisfied it will not be refused. By such a measure the saving of sufferings to the poor, and of funds to the community, would be incalculable. At the same time hospital accommodation for epidemic fever ought to be provided and kept in readiness for any necessity that may occur.

Another point to which I cannot avoid adverting, as being intimately connected with the poverty and destitution of the poor, is their mental and moral degeneration. The intimate connection with, or dependence of vice on extreme poverty, is too obvious to those who have any intercourse with the poor to require remark. To attempt to remove the one, while the other is allowed to remain, in large cities, is to attempt impossibilities. In small isolated communities the poor may suffer great privations and retain their integrity; not so in cities. There, if we expect to remove their mental and moral depravity, we must at the same time improve their physical circumstances. As a member of our county prison board, I have paid some attention to the circumstances of that class of the population who are the inmates of our prisons and penitentiaries. In my visits to these places, I have learned something of the feelings and motives by which they are actuated. I have seen the almost total want of shame and consciousness of guilt which they exhibit, covered, when it serves their purpose, with a thin veil of hypocrisy, which they know how and when to assume. Nor is it wonderful, considering the privations and miseries they endure in their miserable abodes, that they should prefer for a few months (to them) comfortable living in a jail. "There I am wretched, filthy, naked, and ill fed, being in the midst of constant quarrels and broils, without a friend who cares for me. In jail I am well fed, and comfortably lodged; what care I what the world thinks of me, they care nothing for me; though confined, the time will soon pass; and if I can do better, I may; if not, it is only going back again to my old lodgings for a few weeks or months." Such are the reasonings of many of the inmates of our prisons. Most important and beneficial improvements have taken place in our jails within the last ten years, both in the discipline and attention to the morals of the prisoners, and a laudable desire has been manifested for fitting them for returning to society improved in health and in morals. God grant that any thing like a similar desire had been manifested for ameliorating the condition of those whose only crime is poverty, whose dietary is, in the majority of cases, not half so good as that of the inmates of our prisons. I speak of Scotland.

The dietary of able-bodied men in the English workhouse is estimated as equivalent to 24 oz. of wheaten bread in the day, varied as to quality and kind in the different days of the week. In our poor-house it varies from 16 to 20 oz. in the day. In our Infirmary, full diet to those who are convalescent is equivalent to only 24 oz. in the day, unless when extras are ordered by the medical attendant. The lowest rate of diet in our prisons is equivalent to 24 oz. of wheaten bread in the day, the 2d rate to 30 oz., and the highest, or 3d rate, 36 oz. in the day; and it is recommended by Mr Hill, inspector of prisons, that at one meal each day the prisoners should have as much plain food, as porridge or potatoes, as he may wish for, while care is taken that they shall have a portion of exercise daily, lest they contract stiffness in their limbs. The work they are employed in serves also to relieve the tedium of their confinement. They are besides taught to read and write, when their confinement is of more than three months' duration, and are supplied with books for their amusement; and those who are industrious have part of their earnings reserved for them on leaving the prison. The cleanliness and comfort of their cells and bedding is unexceptionable; and in Glasgow, the kindness and attention with which they are treated by Mr Brebner, our excellent prison governor, is universally acknowledged. It must be evident that this state of comfort is not the fate of one-half of our labouring population; and what is the result? The following report of the superintendent and table will explain :—

TABLE IV.

Table of previous imprisonment in the Glasgow Prison from 1st of July 1842, till the last day of June 1844.

	One time.	Twice.	3 times.	4 times.	5 times.	6 times & under 10.	10 times & under 20.	20 times & under 50.	50 times & upwards.	Total.
Males,	337	172	110	90	67	82	14	0	0	872
Females,	182	120	91	67	52	100	48	25	1	686
Total,	519	292	201	157	119	182	62	25	1	1558

Mr Hill, in his eighth excellent report, states, that "at his last visit to our prison, there were 59 boys and 40 girls under 17 years of age, of whom 71 had been in before, 43 of them at least three times, and one of them having been in prison nine times," which he justly ascribes to destitution. The fact that of 99 persons included in the above have been in prison 320 times, giving an average of 3.23 times for each individual; and that the numbers are every year increasing, last year the average number of prisoners being 600; and the additional fact, that many of their own accord apply to be allowed to remain, or to be admitted as inmates; " this fact," as observed by Mr Hill, " shows either that the prison has been made too comfortable, or that there is a large class of persons in a state of severe distress; for which the existing arrangements of society, whether by poor laws or otherwise, afford no sufficient provision." Either of which facts it is important for the interests of the public should be brought to light. He farther observes, " that previous to this report, there were at one time 40 persons, most of them able bodied, undergoing voluntary imprisonment, subject to all the rules and restrictions of the prison." I am much inclined to believe that both of the causes adverted to by Mr Hill have a decided effect in producing this melancholy state of things.

When we consider how much of the destitution arises from the poor being drawn into large cities such as Glasgow and Edinburgh; by there being no proper means of relief provided for them in the counties; in fact, by their being hunted out of the counties to which they belong by the rural police (which seems to be chiefly employed for this purpose, and who in their reports take credit for it), and then taking up their abodes in large cities, by the hopes held out of a greater chance of relief; particularly of getting a night's shelter in the miserable abodes which are kept for that purpose, and by being allowed to beg and prowl through the streets throughout the day; all afford indubitable evidence of the necessity of some efficient provision being made by government, by which the property of the counties should be compelled to support their own poor.

The following abstract, furnished me by Captain Miller, Superintendent of Police, shows the number of these lodging houses within the Glasgow police boundary:—

Total number of lodging houses, . . .	504
Of this number there are situated in the district between Saltmarket and Stockwell Streets, ...	240
In the district east of High Street, comprehending the Vennels, Burnside, &c.	160
In other parts of the city, ...	104

Note.—The above is the number of lodging houses in which promiscuous persons, or nightly lodgers, only are kept.

Police Chambers, Glasgow, 31st Oct. 1843. } (Signed) W. MILLER, S. P.

This table does not include either Calton, Bridgeton, Gorbals, or Anderston, in which it may be presumed the number of lodging houses is equally large in proportion to the inhabitants. For the character and effects of these lodging houses see reports of District Surgeons.

From a table kept by me of the admissions into the Glasgow Fever Hospital in the year 1842, and the country and counties in which they were born, I found that out of 1282 patients admitted, there were only 131 males and 131 females, or about 20 per cent. natives of Glasgow; 558, or near 41 per cent. were natives of Ireland.

Since these sheets were sent to the press, a communication signed by Mr Brebner, Prison Superintendent, and Captain Miller, Superintendent of Police, containing observations and suggestions for the consideration of the Lord Advocate, has been published. The object of these observations is to point out the rapid increase of crime; the increasing number of prisoners beyond the means of accommodation; so as to prevent a proper classification of them; the total number of cells in our prisons being 470, while the number of prisoners, on the 2d January 1844, was 596.

The remedy proposed by these gentlemen is a house of refuge for disorderly and bad characters, in place of sending them to prison for short periods, from which they are no sooner discharged than they return to their old practices.

Would an hour daily on the treadmill, after the second or third conviction, not be a cheaper and more effectual preventative? They state, " that there could easily be collected in this city upwards of 1000 juvenile offenders of both sexes, either known to the police as thieves, or the associates of thieves." It is also suggested that for petty offences a longer period of imprisonment would have a salutary effect. While the prison is more comfortable than their own homes, the propriety of either mode is questionable, and while their ranks are augmented by immigrations of poverty-stricken families from the country, as well as by those brought up in town, under the guardianship of those struggling for existence, nothing else is to be expected, exposed, as they are, to privations as well as temptations of every description. To poverty arising from want of profitable employment, owing to restrictions on commerce, is to be traced the greatest amount of misery and crime. And while it is permitted that more than a fourth part of the earnings of the industrious poor is taken from them to keep up the landlords' rents, the wealth of the sugar lords, and an expensive army of civil and military officers to keep them in order, the community must make up their mind to pay the penalty, or levy an assessment upon the land for their support equivalent in amount to what they they take from the poor.

I gladly embrace this opportunity of returning my most sincere thanks to those medical gentlemen who have furnished me with valuable reports of the state of their districts, and would earnestly recommend to our civic authorities the careful perusal of these reports. From them they will learn the state of danger in which we live, if matters are allowed to continue as they are.

In the year 1832, the proportion of Irish to the whole pauper population, admitted into the fever hospital, was thirty-one per cent.; in 1842 it amounted to forty-one per cent.;—an increase, in ten years, of ten per cent.

I trust that the facts stated will lead to the adoption of such measures by the Government as will relieve that high pressure of misery which is ready to burst asunder the social compact, and by judicious means prevent a catastrophe which may possibly soon occur.

ART. VI.—*On the Structure and Mode of Action of the Iris.*[*] By C. R. HALL, Esq.

THE Iris has ever been a subject of the greatest interest to the physiologist. Yet, notwithstanding the attention which has been directed to it, anatomists are by no means agreed as to the actual nature of this texture, nor, consequently, as to the exact mode in which its actions are performed. And, until the question be settled, we cannot hope to obtain a just comprehension of the laws which regulate the motion of the iris.

The intricacy of the question, and the difficulty of arriving at a correct conclusion, may be inferred from the diversity of opinion which has existed amongst the most eminent observers.

Thus, by Vieussens, Haller, Richerand, Jourdan, and Ammon, the iris is considered essentially vascular, and devoid of all muscular fibre.

On the other hand, Ruysch, Heister, Winslow, Sabatier, Janin, Maunoir, Home, C. Bell, Müller, and perhaps most modern anatomists assume the existence of two sets of muscular fibres,—one for closing, the other for dilating the pupil.

Munro, Meckel, and Treviranus deny the existence of dilating muscular fibres, but admit that of a sphincter pupillæ muscle. Mr Travers and Dr Bolton agree in this, and attribute expansion of the pupil to mere elasticity.

Arnold, after some other German anatomists, supposes a contractile cellular tissue to constitute the agent of motion.

Blumenbach, equally unable to discover muscular fibres in the iris of the white rabbit, or in that of the elephant or the whale, and not satisfied that all the phenomena of the motions of the iris were to be accounted for on the supposition of erectile tissue, was content to consider the structure *sui generis*.

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[*] Read at the Royal Society, Feb. 9, 1843. Communicated by P. M. Roget, M. D., Sec. R. S.

Serres invests the iridal nerves themselves with a special contractile power, by which the changes in the size of the pupil are produced.

Confused by these conflicting opinions, I commenced the investigations which have led to the present communication. It is with diffidence that I venture to offer any observations on so disputed a question, and should my conclusions prove incorrect, I trust that what is presented without confidence, will not be criticised too harshly.

Amongst fishes, in the iris of the cod, haddock, skate, herring, and carp, I have not been able to detect muscular fibre. In the injected iris of the cod the arrangement of vessels is peculiar. On the posterior surface the vessels of the iris, continuous with those of the choroid, form lashes which sweep in a curvilinear course towards the pupillary margin. On the anterior surface, the vessels are disposed somewhat like the *vena verticosa* of the human eye. A number of minute branches are seen to converge to a common point, where a red spot marks out a short trunk passing through the iris to its posterior surface. On removing the crystalline lens, a ring stained with black pigment covers the margin of the pupil for the breadth of about half a line. In appearance both surfaces have the lustre of dead-silver, and are studded with dots, which, on the posterior surface, are mostly black or brown; on the anterior, brilliant gold-coloured spots are interspersed. The larger black dots are stellated. Nothing resembling the ciliary processes of the higher animals is to be found. In shape the pupil is transversely elliptical.*

The pupil of the grayling is pear-shaped, the apex directed forwards, upwards, and inwards.

In the skate the pupil is heart-shaped inferiorly, whilst from the upper portion of the iris a fringed triangular process hangs down like a miniature vine-leaf. There is no appearance of muscular fibre, but the pendant process, and the portion of iris which forms its base, are evidently more vascular than the rest. A bicornate fissure, about a line in width, between the serrated edge of the depending veil and the margin of the lateral and inferior portions of the iris, constitutes the pupil. On opening the eyeball the process may be stretched out so as completely to obliterate the pupillary aperture.

Of reptiles I have only examined the iris of the frog. The pupil is large, rather ovoid, with its long diameter transverse. The anterior surface of the iris is studded with bronze-coloured dots on a brilliant gold ground; the posterior surface is covered with a dark pigment. By the microscope I could discover nothing in appearance or arrangement resembling muscular fibre.

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* Instead of eyelids there is a fold of strong membrane, transparent during life, which is continuous with the sclerotica at the circumference of the front of the eyeballs. It is separated from the front of the cornea by a shut sac lined by a smooth lubricated membrane.

It is well known that the iris is motionless in fishes and some reptiles,[*] and hence we should not expect to find its structure quite analogous to that of other animals, in which its functions are more active.

Of birds, in the pigeon the front of the iris appears smooth and polished; and on the posterior surface, we find no zone of radiating lines separating the iris into ciliary and pupillary portions, as in the mammalia; nor can fibres be traced at all by the naked eye. By the microscope, however, we are enabled to distinguish very fine lines disposed circularly, extending from the margin of the pupil as far as to the attached border of the iris. In the bright yellow iris of the common hen, and in that of the hawk, smooth and homogeneous on both surfaces to the naked eye, by similar means, these circular lines are distinctly seen. The iris of the chick, from its delicacy, and the facility with which much of the yellow colouring vesicles can be washed off, forms a good object for examination;—spread out upon glass, a power of thirty diameters is quite sufficient to disclose what appear to be fibres, arranged concentrically around the pupil, but no longitudinal ones can be detected.

In the goose there is a peculiar vascular arrangement at the base of the iris. Small veins, like the radicles of a creeping root, terminate in a common sinus which runs backwards, receiving in its course, minute vessels from the choroid coat and ciliary processes,—most from the latter structure. After a course of about a line, each sinus terminates by uniting with one or two similar sinuses to form a common trunk. There appear to be four such trunks which pass between the choroid and sclerotica, and receive the veins in which the innumerable small vessels of the choroid terminate.

In the common owl the iris is very broad; and, from the arrangement of its vessels, appears to consist of two distinct portions. The ciliary portion presents a number of long flexuous vessels, which, by their free union, form an elevated line of division at the commencement of the inner portion. From this anastomosis numerous very fine vessels pass obliquely towards the edge of the pupil, crossing each other in their course, so as to constitute an intricate vascular network. In front of the long vessels in the ciliary portion, many smaller vessels pass from the vascular ring, formed by the long ciliary arteries at the base of the iris.

In the interstices between the vessels, under the microscope, transverse fibres may be traced, but they are less distinct than in the chick. At the ciliary edge of the choroid there are small pyramidal elevations continuous at one extremity with many of the small vessels of the choroid, and at the other, with the long flexuous vessels already mentioned as belonging to the iris. I could not satisfy myself whether each little pyramid were a single vascular trunk, or merely an aggregation of small vessels into one fasciculus. The iridal extremity is sometimes connected to one, sometimes to several, of the long vessels. If the latter be veins, each pyramid may possibly be formed by the division and subdivision of the primary vein into several smaller ones, which pursue a straight course for some distance before they ramify in the choroid.

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[*] I concentrated the rays of the sun and of artificial light by a strong lens and threw them on the pupil of the living frog and gold-fish, both in and out of water, without producing the slightest effect upon the iris.

The circular fibres are distinct in the iris of the goose and duck, but neither in these nor in any other birds have I been able to distinguish any longitudinal fibres.

In a very elaborate paper on the Structure of the Iris of Birds, contained in Müller's "Archiv fur Physiologie" for 1837, Dr Krohn of Petersburg states, that, in most species of birds, four laminæ are easily to be distinguished in the iris.

1. The anterior membrane of pigment, on which the colour of the iris depends.

2. The fibrous laminæ.

3. A very thin membrane, which serves as a substratum to the last.

4. The posterior or uveal membrane of pigment.

I condense some of Dr Krohn's observations:—"By a very high microscopic power the anterior pigment is found to consist of colouring molecules, which are more thinly scattered near the pupillary margin; hence, the iris is paler at this part. At the very edge of the pupil they do not exist, allowing a dark ring of uveal pigment to be here seen through the iris. According to Wagner, the yellow colour of the iris of *strix bubo*, and of the common hen, is owing to the presence of a number of yellow oil vesicles collected into heaps. A very thin protective covering is spread over the anterior pigment.

"When the pigment has been carefully washed away, over the whole surface of the iris, from the ciliary to the pupillary margin, we may perceive close parallel fibres disposed concentrically around the pupil as their common centre. They are particularly distinct at the *margo ciliaris*, where they are always broadest, and spread in several sheets, one over the other. Diminishing in thickness as they approach the pupil, at the margin of that aperture, there appears to be but a single layer. All these fibres present the appearance of primitive muscular fascicles, or secondary muscular fibres. Like these, they appear as cylinders with transverse crossings. By the use of alcohol these transverse streaks are rendered more perceptible. Sometimes, especially near the pupil, the fibres have the appearance of the trachea of insects,—but this is an illusion. The fibres of the iris are amongst the smallest of the muscular fibres in general. The thickest of them are in diameter nearest to the muscular fibres of the heart. They are narrowest in birds of prey.

" Maunoir and Muck have described orbicular fibres as seen in the iris of the owl, swan, falcon, crane, and turkey; and Treviranus found near the ciliary margin of the iris of the falcon, fibres running parallel with the circumference of the iris, which, magnified 300 times, appeared striped across like muscular fibres."

Against this testimony of Krohn, it must be stated that Weber failed to distinguish any muscular fibres in the iris of birds; and Professor Owen, though he speaks of a " fine network of interlacing fibres," appears to consider it non-muscular.

No anatomist has professed to have seen *radiated* muscular fibres in the iris of birds. Not a vestige of such fibres can be rendered evident by any mode of examination whatever. The circle at the base of the iris, described by Crampton as muscular, and such I believe it to be, cannot, from its connections, serve to dilate the pupil, and cannot therefore be the analogue to the dilating muscle which some assume to exist in mammalia. Yet the activity of the iris in birds is exceedingly great. Compare the ox with the parrot, for example. Were a muscle to dilate the pupil necessary, we should have expected to find it strongly developed in birds.

The distribution of the iridal nerves is well seen in the goose and the owl. Five or six nerves diverge from a point near to the entrance of the optic, and, arrived at the ciliary body, they separate from each other and pass in arches around the circumference of the iris, giving off minute filaments to that structure.

Of mammalia,—in the rabbit, hare, cat, dog, squirrel, pig, sheep, and ox, as a general remark, the anterior surface of the iris, however varying in colour, is smooth and shining; and the posterior surface covered with dark pigment, and narrower from its free to its attached margin than the anterior, in consequence of the attachment of the ciliary processes to the periphery of the iris. On the pigment being washed off, a full view of the converging radii is exposed. They are best seen in the iris of the ox. By the abrupt termination of these radii, is formed a line of division between the ciliary and pupillary portions of the iris. The relative size of these two portions depends principally on the shape of the pupil, and on the degree in which it was capable of being altered during life. When the aperture is circular, the pupillary portion is nearly of equal breadth, and the radii of equal length throughout, except that they are slightly shorter on the nasal side, rendering the iris rather narrower at that than at any other part of its extent. We find also the circle of ciliary processes narrower here than elsewhere.

As seen after death, in the dog and hare for instance, of the entire breadth of the iris, the ciliary portion may be three-fourths, the pupillary the remaining fourth. When the pupil is transversely oval, as in the sheep and ox, the iris is broader above and below than at the sides, the radii being longer, and the pupillary portion much broader, so that, when flaccid and stretched out, the margins above and below form two convex lines, which overlap each other so as to obliterate the pupil. In the cat, in which the long axis of the pupil is vertical, just the reverse is seen; the lateral portions of the iris much exceed in breadth the upper and lower. During life the pupil of the cat is round in the dusk, and more or less vertically elliptical in daylight. This variety of shape is owing to the comparative narrowness of the upper and lower portions, rendering these, after contraction has made them tense, incapable of further yielding, and consequently "points d'appui" for the remainder of the iris. In the larger varieties of the feline tribe, the pupil is said to be round in every degree of light: in them, it is probable, the above anatomical arrangement does not exist.

In the pupillary portion of the iris of each of the animals above mentioned may be traced very fine lines running parallel to each other, and disposed concentrically around the pupil. They do not appear to extend nearer to the ciliary edge than the apparent line of division already mentioned. In the thick dark-brown iris of the ox, the colour renders the object unfavourable for microscopic examination; but, after prolonged maceration, when stretched out and dried upon glass, the circular fibres are apparent to the naked eye. They pursue a somewhat wavy course, occasionally crossing so as to appear to interlace one with another, as Lauth has also observed. From the large diameter of these evident fibres, as compared with the microscopic ones of the more delicate irides of many other animals, they can hardly be supposed to be the primary muscular filaments, but rather as pointing out the arrangement of these, of which many probably are bound up in each of the very perceptible brown lines. The convergent folds which form the radii on the uveal surface of the iris appear to be membranous plicæ, continuous with the ciliary processes, and have nothing approaching to muscular fibre in their composition. It is difficult so to remove this membrane as to ascertain the exact nature of the remainder of the iris in the ox; but there are certainly no distinct radiating muscular fibres to be detected. On dipping an iris, previously macerated and cleaned, into dilute nitric acid, faint white flexuous lines become visible on the anterior surface of the pupillary portion. Nearer to its attached than to its free margin, on the front of the iris, may often be noticed a series of concentric wrinkles, which disappear when the iris is expanded. I have frequently observed these in the eyes of living horses, oxen, dogs, and cats, and in men and children. In the relaxed iris of the ox after death, the appearance is particularly marked, from which circumstance must it have happened that Dr Monro stated that he found on the anterior surface of the iris of the ox "a broad, flat, oval organ, with fibres of a dark-reddish colour, disposed in nearly the same manner as those of the *orbicularis palpebrarum* are;" and this he considered and figured as the "sphincter of the pupil." (On the Eye and Ear, 1797, p. 111.)

In the living eye we do not see the whole extent of the iris. The iris extends at its circumference considerably beyond the junction of the cornea with the sclerotica. If a vertical section be made of an eye hardened in alcohol, it will be seen how much the attachment of the ciliary processes to the iris diminishes the extent of free posterior surface as compared with the anterior. This is most evident in the ox and sheep.

The vascular ring formed on the front of the iris by the long ciliary arteries is rendered very distinct in the light-coloured iris of the dog, cat, rabbit, or hare, on dipping it into dilute nitric acid, by which the contained blood is made brown. Small tortuous branches pass off from this iridal coronary artery, and freely supply the structure above and below, as is well seen in the thin semitransparent iris of the kitten.

Besides the radiated plicæ of the uvea, there are fine wavy filaments, which pass at irregular distances from the attached towards the pupillary edge, and most distinct when viewed from the front. They seem to pursue no determinate course, and often divide and subdivide.

On examining the human iris during life, with a light sufficiently strong to induce moderate contraction of the pupil, we observe a number of elevations, most prominent at about a third of the breadth of the iris from its pupillary margin. Whitish and distinct in blue and grey eyes, brown, and more like small tufts in the hazel iris. From the outer part of these eminences, numerous striæ pass towards the ciliary margin. Arranged in sets, the striæ commenced at the circumference of the iris in a fan-like manner, and gradually converged, until they form by their union one of the prominences referred to. These prominences vary much in shape and size; for the most part connected to each other, they form an irregular, but not always continuous ring. From the inner edge of this ring, but less prominent than the fan-like outer striæ, very fine parallel lines may be traced to the pupil, where a narrow ring, so dark as to escape notice on cursory examination, forms the most internal boundary. In some irides, generally the brown, the striæ may be noticed, like delicate threads passing from the base of the iris towards the pupil, without the usual kind of prominences, but in their stead a few minute tufts, darker coloured than the rest.

Bertrandi describes these as vessels; Zinn as fibres; Haller denies their muscularity, but gives no decided opinion as to their nature; on these "*flocculi radiati*," as he terms them, he considers the colour of the iris mainly to depend, though the *pigmentum nigrum*, and the vessels and nerves, may assist in producing the effect. Dr Jacob considers them muscular, and likens them to the *carneæ columnæ* and *cordæ tendineæ* of the heart, "both in form, arrangement, and irregularity of conformation." Jourdan and Mr T. Wharton Jones have supposed them to be "the divisions of the ciliary nerves." It is very difficult to isolate and separately examine one of these striæ. On exposure to the air they shrink, until scarcely the outlines of the former projections remain. Under the microscope they appear to be tortuous threads, dividing near the pupil into two or more smaller threads, which separate and are lost amongst the pupillary fibres. In appearance they have nothing in common with muscular filaments; what seems to be their ultimate constituent, is larger, and has no transverse markings. I have never succeeded in tracing their direct continuity with the ciliary nerves; but from the difficulty of obtaining the human eye sufficiently fresh, I have never been able to trace in it the ciliary nerves, farther than the *corpus ciliare*. Considering the large supply of ciliary nerves which enter the ciliary body, all round the attachment of the iris, we should *à priori* expect their distribution to be plain and visible; but unless we admit that we have this in the manifest plexiform striæ of the anterior surface, we shall scarcely find traces of their destination. Were these striæ actually muscular agents for dilating the pupil, we should have them in birds and other animals, as well as in man; but they are not to be found. In the ox I have traced some of the ciliary nerves through the ciliary body into the iris, in the tissue of which they ramify and join each other freely.

The reasons for considering these striæ non-muscular are,

1. In appearance they are unlike muscular fibre.
2. They do not exist in many animals which have the power of dilating the pupil.
3. Their distribution in man is very irregular, in some being very distinct and prominent, in others scarcely visible; whilst there is no corresponding difference in the activity of the iris.
4. The prominences being often by no means equally arranged

round the pupil, one would suppose any contraction must, *cæteris paribus*, be greatest in the largest prominence formed by these striæ, and would consequently dilate the pupil unequally, by dragging most towards that point.

5. Were they muscular, they would be in action during dilatation, and should consequently then appear shorter and more projecting; but in reality they are, as Haller observed, much more flexuous during dilatation than during contraction of the pupil.

That they are not vessels, Dr Jacob argues from "the thickness of these striæ being so much greater than the vessels of the iris; and from their being arranged in a manner altogether different from vascular inosculation;" to which we may add, that injection of the iris leaves these striæ as before. But although not themselves either single vessels, or entirely made up of a congeries of small ones, the membrane which invests them in front is so excessively vascular, that when examined under the microscope by a power of 250, the focus being exactly right for the most superficial part, nothing but minute vessels is visible.

If the human iris be detached, and carefully floated about in water until all the pigment is removed, and then stretched out on glass, when dried, it is sufficiently transparent for the microscope. I give the results of the examination of uninjected irides, as Soëmmering's beautiful engraving affords a just representation of a well-injected iris, and the minute anatomy of the structure, independent of the vessels, is obscured by injection. When dried in the way I have mentioned, the outlines of the anterior striæ and those of the posterior radii and vessels are seen on both surfaces, though not with equal distinctness. A narrow ribband-like band is seen to surround the pupil on the posterior aspect. The eye alone can scarcely distinguish in it 'the very delicate fibres which the microscope renders evident. They are extremely fine, of a faint yellowish-ash colour, and form a very thin but defined layer, which lies upon, and posterior to, the pupillary portion of the iris. In diameter, they are considerably less than the anterior striæ, from which they totally differ in appearance. These minute concentric filaments appear solid, and present the most striking resemblance to the fibres which surround each of the two suckers placed on the lower surface of the transparent *agulus foliaceus*, and which, from situation and appearance, may be considered as muscular rings. In the remainder of the iris I have not been able to discover anything like muscular fibre. Vessels of equal calibre anastomose together so freely as to form a most intricate and beautiful network. Here and there a few disks of blood-globules are seen remaining in the blood-vessels. There is no distinction of arteries and veins, but an excessive universally-communicating vascular web.*

I have not succeeded in detecting the villous character which, according to Dr Ammon, distinguishes the membrane of the anterior surface of the iris from the smooth, translucent tunic which lines the cornea. Mr T. W. Jones states, that "a continuous membrane lining the anterior chamber of the aqueous humour, cannot be demonstrated. A membrane lining the posterior chamber of the aqueous humour is more demonstrable." Dr Ammon, on the contrary, could find no serous membrane on the posterior surface of the iris. Henle could discover no epithelium on either surface of the iris, or on the capsule of the lens; whilst by Valentin it is described as existing on both surfaces. Mr T. W. Jones has found "epithelic corpuscles on the anterior surface of the anterior wall of the capsule of the lens in the lamb's eye." The result of microscopic examination being thus inconclusive, we may at present assume, from the shining polished aspect of the interior of the anterior and posterior chambers of the aqueous humour, that both are lined throughout by the same delicate serous membrane. It is unlikely that different parts of a shut cavity, even in contact with the same secretion, should materially differ in organization.

On the posterior surface of the iris, we perceive the delicate radii which are the analogues of the very perceptible plicæ on the urea of the ox. As Dr Jacob states, they certainly appear to be of the same nature as the ciliary processes. If the ciliary processes and iris be well washed until quite free from pigment, and then dipped for an instant into dilute nitric acid, each ciliary process appears to consist of a central stem, which sends out on each side a great number of extremely minute branches. Under the microscope, this arborescent arrangement is very beautiful, and the vascular connection between the iris and the ciliary processes very apparent. No muscular fibres are perceptible in the ciliary processes.

Mr Lawrence remarks, that the vessels of the iris do not appear to contain red blood: "at least," he continues, "when it is wounded, no blood flows." I have seen red blood effused into the anterior chamber when the iris has been accidentally wounded by the couching needle; and from the disks of blood-globules seen in the iridal vessels under the lens, it is presumable that red blood circulates through the iris during life. Indeed, in the living human albino, a simple lens will disclose minute red vessels; and in the white rabbit, these may be seen on careful examination with the naked eye.

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* By slightly deviating from the exact focus, a number of natural-looking but false appearances may be obtained. One may thus make out that the iris consists entirely of fibres, circular, oblique, or radiating. By using a very high power, the proper focus for the most prominent will convey an erroneous impression of the deeper-seated parts, and thus we see at one moment only the superficial layer of anastomosing vessels without any filaments whatever; or, at another, an outline of the anterior striæ obscured by the superjacent vessels. The degree of illumination also materially modifies the appearance presented. The lens should be achromatic, and the lower the power that will suffice the better, as it allows of a more extensive view without change of adjustment than the higher powers. A moderate light is best for examining the concentric pupillary fibres; whilst the anterior striæ are most distinct when a portion of iris is moistened with water, and viewed as an opaque object.

From anatomical investigation, then, I conclude that the iris is composed of an exceedingly vascular tissue, connected by vessels to the choroid, ciliary processes, sclerotica, and cornea; richly supplied with nerves, which in the human iris are probably seen in front as thread-like striæ; invested on both surfaces by the membrane of the aqueous humour, and more or less thickly covered with pigment, which, on the anterior surface, by its varying colour, imparts the characteristic hue to the iris; on the posterior surface, by its darkness, renders an otherwise semitransparent structure opaque;—of a layer of concentric muscular fibres situated on the posterior surface of the pupillary portion of the iris in man and mammaliæ, extending much nearer to the ciliary margin, and therefore forming a much wider layer in birds, and not existing at all in fishes and some reptiles.

How do these conclusions accord with physiology?

From the hypothesis of the French academician, Mery, who contended that contraction of the pupil resulted from an influx of animal spirits into the longitudinal fibres;—or that of Weitbrecht, who stated that the pupil was contracted when the iris was drawn towards the crystalline lens, and dilated when drawn back towards the cornea;—to the principal theories of the present day, most attempts to explain the action of the iris have been vague and inconclusive.

It was the opinion of Haller that the iris consisted of erectile tissue, which became injected when stimulated by the action of light upon the retina. Richerand thus expresses this view: "To explain the motions of the iris—he writes—it is not necessary to admit that muscular fibres enter into its structure; it is enough to know its vascular, spongy and nervous texture; the irritation of the retina sympathetically transmitted to the iris, determines a more copious afflux of humours; its tissue dilates and stretches, the circumference of the pupil is pushed towards the axis of this opening, which becomes contracted by this vital expansion of the membranous tissue. When the irritating cause ceases to act, by our passing from light into darkness, the humours flow back into the neighbouring vessels, the membrane of the iris returns upon itself, and the pupil enlarges the more as the darkness is greater." As an instance of contraction of the pupil from distension of the vessels of the iris, M. Renard found, whilst instituting experiments upon dead bodies for the purpose of ascertaining the effects of compressing the diaphragm upwards by the development of gas in the abdomen, amongst the signs of fluid sent to the head "resserrement de la pupille;" and this is adduced as a fact supporting the theory of erectility. But, assuming it to be a fact, this, as well as the contraction of the pupil which follows successful minute injection of the eye, only proves that a lax tissue becomes expanded when its numerous vessels are distended, and not that the rapid and delicate movements of the living iris are owing solely to alternate emptying and distension of its vessels. The contracted state of the pupil in iritis has been adduced as supporting the opinion that contraction in the normal condition depends upon an augmented influx of blood. In iritis, I believe that the contraction of the pupil is usually in proportion to the degree in which the retina has become morbidly sensible. The intolerance of light, and not the amount of increased vascularity, seems to be the index to the degree of contraction of the pupil.

In syphilitic iritis, where there is often but little intolerance of light, the pupil is usually larger than in the other varieties.

It is evident that no argument, either for or against the erectile theory, is deducible from the phenomena of iritis. The absence of *arteriæ helicinæ* in the iris does not prove that its structure is not erectile, since, should the accuracy of Müller's views be confirmed, it would be difficult to account for all the phenomena of erection, merely by the existence of this peculiar set of vessels. There is then no argument from anatomy to disprove the erectility of the iris. Nor is it, perhaps, a valid objection that we have no instance of erectile tissue being so instantaneously distended and emptied as would be requisite to effect the very rapid motions of the iris. The equal calibre and exceedingly free anastomoses of the iridal blood-vessels would permit very quick changes in the quantity of their contents, and we may assume that a little increase in the delicacy of action of that agent which causes the sudden suffusion of the face in blushing, and the equally sudden pallor of fear, would suffice to produce even the rapid and varying oscillations of the iris.

The principal reason for not ascribing the motions of the iris solely to erectility, seems this, viz. it has not been proved that distension of erectile tissue can be induced immediately after death by irritation of the nerves supplying it. But it has been proved that the action of muscles in general, and that the contraction of the pupil may be thus excited.

In reference to the muscularity of the iris, the mode of action of belladonna and some other narcotics in producing dilatation of the pupil merits inquiry. To ascertain with precision the phenomena which really attend the external application of belladonna to the eye, I selected a woman who had complete lenticular cataract but active iris of one eye, and perfect vision with the other, in order that the effects might not be complicated from light being admitted to the eye which was not under the narcotic influence.

Extract of belladonna was freely applied at the distance of an inch around the sound eye. An hour afterwards, no effect. Strong watery solution of the same extract was then dropped into the eye, and cloths moistened with it kept applied to the eyelids. In half an hour the pupil had become fully dilated, and the pupil of the cataractous eye diminished; but when the sound eye was closed, the pupil of the other—non-narcotized eye—enlarged to the size which it had presented before the application of the belladonna to its fellow.

1. There was a sensation of numbness and tension of the whole surface to which belladonna had been applied, and the eyeball felt stiff.

2. Bright light caused a disagreeable dazzling sensation, but no change in the size of the pupil.

3. The attempt to examine near objects caused slight vertigo, and a confused feeling in the head.

4. When beyond a certain distance, objects appeared distinct and of their natural colour; but as the distance was diminished, the objects became dim and confused, appeared longer than their real size, and were invariably of a slightly red colour.

5. The disorder of vision lasted for about twenty-four hours.

From the more speedy action of a solution dropped into the conjunctiva, it would seem that belladonna must be absorbed before its action becomes apparent. It is possible that belladonna causes dilatation of the pupil by a reflex action through the brain, the extremities of the fifth nerve being affected, as proved by the tingling and numbness of the eyelids. It is certainly not by any reflex action merely through the lenticular ganglion, as some have supposed, for the pupil of the cat may be dilated by belladonna dropped into the eye, and in the cat there is no direct communication between the fifth nerve and the ophthalmic ganglion. It appears, therefore, more probable that the action of belladonna is direct, and upon the filaments of the motor nerves themselves, which it reaches by endosmose. Did it act through the sensorium, we should expect that both eyes would participate in the effect, just as light thrown upon one eye causes contraction of both pupils. As vision is quite perfect when the object is sufficiently distinct, the retina cannot be directly affected. There is not more disturbance of vision than may be accounted for by the enlargement of the pupil. That belladonna does not necessarily act through the medium of sentient nerves may be inferred from the experiments of Müller, who found that the application of narcotic solutions to isolated motor nerves destroyed for a time their conducting power. Mr T. W. Jones conceives that belladonna may act "by diminishing for a time the general sensibility of the retina conferred upon it by the fifth nerve." This takes for granted the assumption that it is by virtue of its general or common, and not of its special or visual sensibility, that the retina influences the iris, a premise certainly not established.

Those cases in which poisonous doses of belladonna taken internally have caused contraction of both pupils, have been adduced by Mr Dalrymple to support his view that belladonna, in small doses, or as an outward application, acts as a stimulant to the radiating muscular fibres, and, by exciting their contraction, enlarges the pupil. As explaining how a very large dose causes contraction of the pupils,—the circular fibres being stimulated to act, though the dose has been so great as to destroy the activity of the radiating ones, the latter are assumed to be voluntary, the circular fibres, involuntary; voluntary muscles being known to evince the effect of derangement of the nervous centres sooner than involuntary ones.

Is it, then, by virtue of its voluntary power that the supposed *dilator pupillæ* maintains the pupil so large in compression of the brain, and in the last stage of hydrocephalus? It appears too great an assumption in those who admit two sets of muscular fibres, to suppose one set exclusively stimulated to action by belladonna, when we find the same effect produced by injuries attended with an abolition of all voluntary motion. Analogy, moreover, would lead us to conclude that belladonna temporarily impairs, rather than excites muscular contractility. As a suppository, belladonna relieves tenesmus; applied freely to the cervix, is said to allay painful spasmodic contractions of the uterus;—in both cases, it acts probably by lessening at once morbid sensibility and muscular irritability. Dr C. B. Williams found, of different narcotics, that extract of conium and extract of belladonna impaired the contractility of the air-tubes most completely. Conia was found by Dr Christison to destroy for a time the irritability of muscular fibre.

Knowing that all the sensations of vision may proceed from internal causes; that luminous spectra occur in amaurosis, may be excited by galvanism, and have been remarked even after extirpation of the eyeball; and especially that narcotics, by their circulation through the brain, may excite the appearance of sparks before the eyes;—it does not appear irrational to refer the peculiar effect of opium, and occasionally of large doses of belladonna, to some irritation at the root of the optic or third nerve, or both, produced through the medium of the cerebral circulation, which we have sufficient evidence is greatly disturbed. Any such irritation acting through the motory filament of the third nerve might induce contraction of the pupil. It is no argument against this, that the orbital muscles are not thrown into action at the same time, because the iris, from its exquisite organization, and peculiar situation in a fluid medium, may well be supposed to indicate, by its action, changes in the brain, insufficient to excite other coarser muscles. Otherwise, there could never be change of the size of the pupil without some motion of the eyeball. In a case of ptosis and eversion of the eye, evidently from palsy of the third nerve, —the result of an injury to the head,—the fixed and largely dilated pupil has now begun to act a little, although the orbital muscles are still perfectly powerless. This is the usual mode by which we receive an intimation of ultimate recovery in these cases.

It has been stated that, in paralysis of the iris, there is not dilated pupil, but that the iris is then relaxed and vacillating like any other paralysed tissue. From which, the conclusion that belladonna does not paralyse the muscular fibres of the iris at all. The remark, however, seems too unconditional, and the conclusion erroneous. When the arm is suddenly palsied in hemiplegia, its muscles do not at first feel "relaxed and vacillating." In palsy of the third nerve, when we must admit the iris to be paralysed, we do not usually find a pupil of medium size with relaxed and vacillating iris, but a fixed and dilated pupil. Tremulousness of the iris is probably owing more to the condition of the humours than to that of the iris itself.*

It does not seem to have been satisfactorily explained why, if these do exist, (dilating fibres), these should be unaffected by belladonna, which has certainly the power of destroying, for a time, the contractility of the circular fibres.

Another objection, assigned by Dr Bolton, is the permanent dilatation of the pupil in amaurosis. "Now, can it be conceived," he asks, "that a muscle can remain for twenty, thirty, or forty years in a state of constant contraction? The idea is totally inadmissible." But there are cases of amaurosis in which the pupil is small; in these it might be said with equal justice to be impossible for the *sphincter pupillæ* to remain contracted for so great a length of time.

The most forcible argument appears to be, that no one has hitherto succeeded, by direct irritation of the iris itself, or of the nerves supplying it, either by mechanical injury or by galvanism, in producing dilatation of the pupil, whilst every well-conducted experiment on the subject shows that irritation of the third nerve excites contraction of the pupil, at the same time that it often throws some of the muscles of the eyeball into action.* Strychnine is known to affect the action of muscles. By friction with strychnine over one eyebrow the pupil of that eye becomes much contracted.

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* That the iris is not necessarily palsied when it is of medium size, and relaxed and vacillating, is proved by the following case:—A woman, aged 40, has been frequently struck over the left eye by her husband. She does not know that she has ever suffered from any inflammatory affection of the eye, but, for several years past, its visual power has been much impaired. The iris is tremulous, the pupil rather large, and the eye of little use for vision. On sudden exposure, after keeping the eye covered, the pupil contracts slightly. Belladonna materially increased the dilatation of the pupil, and diminished the vacillation of the iris.

Fontana, Zinn, Porterfield, and others, have supposed the iris to be in its state of rest during contraction of the pupil, chiefly from the circumstance of that being its condition during sleep, when most parts are quiescent. This opinion is at once disproved by the effect of irritating the motor nerve, and by the greater size of the pupil after death than when contracted during life. Immediately after death the pupil retains for a certain time the magnitude it possessed at the last moments of life, and may be widely dilated, as is common after death by hanging or drowning; or small, as after death from concussion of the brain. After all vitality has left the body, the pupil is "neither much contracted nor much dilated." This is the most passive condition of the iris, being that which its mere elasticity causes it to assume in the absence of all vital action. Mr T. W. Jones proved, by a very simple and ingenious method, that the elasticity of the iris acts towards, as well as from, the centre of the pupil. By placing the points of a pair of dissecting forceps in the pupil, and allowing them to separate so as to stretch the iris, on their withdrawal, the iris immediately resumes its former size and figure. If the iris when fresh be stretched out from its ciliary attachment, it will immediately recoil on removal of the force. These are all the effects which are produced by mere elasticity, and are alike met with in the irides of man and other animals, provided the structure has not become putrid, or been immersed in liquid.

The relaxation of the sphincter fibres would occasion, therefore, merely a medium degree of dilatation, were physical elasticity the only antagonist. And the same elasticity would equally oppose extreme dilatation and extreme contraction. There must, consequently, exist some other power for dilating the pupil. That this power is not muscular in its nature, I conclude, because its phenomena differ from those of muscular action in the following respects, viz.

1. Dilatation of the pupil is not excited by stimuli; the dilating agent, therefore, is not irritable.

2. Dilatation is greater in proportion as the natural stimulus of the eye—light—is less.

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* I do not forget that Magendie found division of the *fifth* nerve to be followed by dilated pupil in the dog and cat,—but his experiments were exceptionable.

3. It is present in a high degree when muscular power in general is lowered; for example, in apoplexy and compression.

4. Also, under the influence of narcotics which impair muscular contractility in general.

If, then, mere elasticity would not suffice, and radiating muscular fibres do not exist in the iris, what is the agency by which dilatation of the pupil is effected? Weber considers that, to explain the motion of the iris, there is need neither of circular nor of radiating fibres, but only of the spongy texture of the iris endued with contractility, the external and internal zones of the iris contracting not at once but by turns.* But there is no real evidence of any such alternate action of the two zones. As far as can be observed, the whole texture of the iris moves, though not in equal degree, during both the enlargement and the diminution of the pupils. Perhaps the amount of our knowledge respecting the contractility of the cellular tissue is not sufficient to justify our denying the possibility of its being influential in causing motion of the iris. The same argument which was brought forward against the theory of muscle being the agent of the two motions of the iris, will equally apply to the supposition that both diminution and enlargement of the pupil are due to the contractility of cellular tissue, or indeed to the action of one and the same agent of what description soever, viz. that were the agent of dilatation the same as that of contraction, we should be able to induce artificially the one state as readily, and in the same manner as the other, by exciting some of the ciliary nerves. In order to produce effects so diametrically opposite, the filaments of the cellular tissue of the iris would have to be arranged in two distinctly different ways, and of this we have no evidence. If, then, the cellular tissue of the iris do possess a superior degree of contractility—a vital endowment ceasing with life—by a closer approximation of its filaments under action, there would result a lessened surface from the drawing of the free towards the attached margin, and consequently dilatation of the pupil.

In the erectile tissue which is open to observation, the vessels become expanded and the structure consequently enlarged under irritation, but the vessels do not contract beyond their natural standard, the structure does not become smaller than its usual size, on the subsidence of the stimulation. The iris, however, does contract beyond its natural size when the pupil is fully dilated, so that if it consisted solely of erectile tissue, the functions of this kind of organization would in this instance be modified. Yet still the great vascularity of the iris, of the choroid at its base, and of the ciliary processes in mammalia, the vascular plexus in front of the iris of the seal and crocodile, the existence of the pecten in birds, and of the choroid gland in osseous fishes, would certainly lead one to suspect that such an excessive supply of blood-vessels was subservient to something more than the mere nutrition of the eye. Mr Toynbee's observations on the assimilation of non-vascular textures, render it probable that the close packing together of small blood-vessels in some of the ocular textures, is to compensate for the absence of them in certain other of these structures, which are nourished by absorption of a nutritious exudation from the neighbouring very vascular parts. He instances the cornea, crystalline, and vitreous humours. But whilst subservient to this end, the great vascularity may likewise fulfil a second intention, and we cannot thus account for the whorl-like arrangement and excessive number of the choroideal veins, placed between membranes nourished in the common way. The choroid vessels are perhaps influential in producing the focal adaptation of the eye, and it may be a question worthy of consideration, whether the pupil is not fully dilated in consequence of the sudden emptying of the vascular network of the iris by virtue of the vital contractility of the small blood-vessels. The minute vessels of the iris being filled with blood during contraction of the pupil, as soon as the stimulation of the retina, and consequent action of the circular muscle of the iris, have ceased, contract and eject more or less of their contents in proportion as the antagonising sphincter, or, as Krohn terms it, *tensor iridis*, is relaxed, and thus the pupil becomes enlarged. It is unnecessary here to adduce proofs that all the smaller blood-vessels do possess a vital contractility; the empty state of the arteries after death, the paleness of the surface, and the termination of inflammation by resolution, could hardly be otherwise explained. Whilst examining the eye of a living white rabbit through a simple lens, I could distinctly trace vessels full of red blood; when the iris was expanded, the vessels were straight; when the pupil was dilated, the vessels appeared tortuous; but the moment that life became extinct, they all instantly disappeared. I cannot judge from their appearance that these vessels contained more blood in the one than in the other condition, but it is in vessels of smaller calibre than these that I am assuming the power to reside. Whether the capillaries owe their characteristic property to the organic nervous system, is a question of great interest, but not peculiar to the subject under consideration. The vital action, whatever it may be, which causes expansion of the pupil in some cases, lasts for a certain time after death. The period of its continuance, however, may be shortened; if the cornea of a man just dead, whose pupils are large, be punctured, and the aqueous humour allowed to escape, the pupils become of the medium size. In the operation for cataract by extraction, belladonna having been previously applied, on the escape of the aqueous humour. The ring-like iris immediately expands, so as to diminish the size of the pupil, and sometimes to obstruct the passage of the lens. In this case, however, the pressure of the muscles of the globe may tend to push the posterior humours against the uvea, and thus cause the iris to project and lessen the pupil. Pressure on the eyeball with the finger in practising the operation on the dead subject will often produce this effect.

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* Tractatus de Motu Iridis, 1821, p. 43.

If the vessels play so important a part in producing dilatation, what physiological grounds have we for not ascribing contraction of the pupil to them likewise, rather than to a special sphincter muscle? That nervous influence has the power of causing distension of the capillaries is evident from blushing, from the patch of vascularity which often appears over the extremities of the affected nerve in neuralgia, and especially from erection; but we know not whether it is by addition to, or subtraction from the nervous energy of the vessels, that these results are obtained. Dr Billing argues, with his usual ability, that the capillaries of a congested part are enlarged from receiving only a diminished supply of nervous energy. Be this as it may, we have no instance of vessels being distended through the agency of a muscular nerve, enlarging where such nerve is stimulated, contracting when the stimulation ceases. The third nerve is galvanised; different muscles of the eyeball are thrown into action, and the pupil is diminished in size. Is it likely that certain filaments of the same nerve should excite the vessels of the iris to enlarge, and the remaining filaments cause contraction of the fibres of muscles? And yet this must be assumed, unless the existence of a special muscle for contracting the pupil be admitted. The combination of vascular action and muscular contractility for the performance of the function designed, is exemplified in Houston's muscle, and (according to Houston) in the apparatus for protruding the tongue of the chameleon: hence, what I have surmised with regard to the iris is not against all analogy.

It is difficult to explain why the pupil is so small in the absence of light during sleep. But the difficulty is not lessened by denying the muscular nature of the agent which causes contraction of the pupil. On the contrary, whilst we have no instance of erectile tissue,—the mind being at rest,—naturally becoming turgid during sleep, Dr Mackenzie remarks, on the authority of Weber, "that the *sphincter ani* and sphincter of the bladder resist the feces and the urine more during sleep than while we are awake; and from this cause, the bladder often becomes distended during the night, to a degree far beyond what it can endure during the day."

Haller objected to the existence of a sphincter muscle of the pupil, that he could not understand how the shape of the pupil of the cat could be altered with regularity by such means. If it were necessary to consider each ultimate muscular filament as a complete circle, it would be difficult to explain the diversity of shape presented by the pupil in different animals; but viewing each filament as only of a certain length, the whole number being so arranged as to act concentrically, the objection disappears. Any portion of the iris which was narrower than the rest, after it had acted to its utmost, would become a fixed point for the other portions to pull from. A single such fixed point would give a cordate; two opposite points an elliptical; three a triangular pupil; and thus may we account not only for the variety of the natural shapes of the pupil, but also for the still greater variety seen in the human eye as the result of disease.

It has been said that there is no muscle in the body without an antagonist, and that we ought not, therefore, to admit in the iris the existence of contracting without that of dilating muscular fibres also. The contractions of the heart are only antagonized by the blood-vessels, and by the elasticity of its own structure. Just so, may we suppose the contraction of the *sphincter pupillæ* to be antagonized by the vital contractility of the blood-vessels, and by the elasticity of the iris.

Another objection raised, is that, if muscle be the agent of the movement of the iris, we have no other example in the body of muscular fibres undergoing such changes of length as would be necessary in this instance. But the muscular fibres of the urinary bladder and alimentary canal are considered by some to undergo quite as great changes in the contraction and dilatation of these hollow viscera.

On physiological grounds, I infer,

1. That contraction of the pupil is effected by a special muscle through the medium of a motor nerve.
2. That this is the only muscle of the iris.
3. That the agent by which dilatation of the pupil is effected has not yet been satisfactorily ascertained; but, as a mere conjecture, we may suppose it to consist in an unusual degree of vital contractility, either of the cellular tissue, or else of the small blood-vessels of the iris.
4. That the iris accommodates itself to changes of size, and tends to return from extremes, both of contraction and dilatation of the pupil, by virtue of its elasticity.
5. That, beyond this, mere elasticity has no part in causing motion of the iris.

Notes.

1. *On the Pupil of the Skate.*—In the skate the shape of the eyeball is very unusual. Situated laterally on the dorsal surface of the head, direct-

ed forwards and slightly outwards, it is uncovered superiorly by any cartilaginous wall. A spherical shape would have caused considerable projection; hence, the eyeball is flat superiorly, and here alone is the sclerotica—elsewhere semi-cartilaginous, thin and pliant. Into this softer part of the sclerotica is the rectus superior muscle inserted.

A spherical lens requires a concave nervous screen. Were the pupil round, it would admit rays of light, which, by impinging on the upper portion of the retina,—not concave like the rest, but flat,—would confuse vision by forming an irregular image. This is obviated by the palmate process of the iris obstructing exactly those rays which would so impinge. Supported on a cartilaginous pedicle, which is hollowed out at its extremity to receive the smooth cartilaginous knob of the posterior part of the sclerotic, the eyeball moves on its axis, with all the facility that a somewhat similar ball-and-socket joint affords to the vertebræ of the fish. At the same time, this pedicle would oppose any retraction of the eyeball, during the action of the straight muscles. Unable to recede, the elastic tunics of the eye are probably slightly compressed by the combined action of the recti muscles; the superior flattish wall of the globe being the most pliant, would yield most, added to which, the contraction of the superior rectus would tend to raise its central portion; the pressure, equal in all directions, of the fluid vitreous humour, would distend equally this upper wall, and render its inner surface more concave. From the intimate adhesion between the two, any elevation of the upper wall of the globe will cause a corresponding elevation of the upper part of the iris and of its vine-leaf-like process, thus enlarging the pupil and admitting luminous rays to a part of the retina to which, at other times, they have not access.

From the eyeball tending to yield so much more readily at one part than at the rest, the effect of such compression as that exerted by the recti when in action,—viz. that of drawing the anterior towards the unreceding posterior wall,—would be to shorten the antero-posterior axis, and, at the same time, to enlarge the pupil, thus adapting the eye for seeing distant objects. That it is by this or some analogous means that the slight degree of adaptation for focal distances, or for different densities of the surrounding medium, required by the fish, is accomplished, is further presumable from the absence of the choroid gland in the eye of the rays and other cartilaginous fishes. The superior rectus acting singly, would raise the upper yielding part of the sclerotica, and with it, the iridal veil, before it turned the eyeball itself upwards on its socket, and in this way allow a greater number of rays of light from above to enter the eye, without any change of posture of the fish, than could otherwise gain admission. The habit of the rays to lie at the bottom of the water seems to render this view not improbable.

If the above remarks be correct, the iris in the skate serves as a passive diaphragm, itself moved indirectly by the agency of the orbital muscles, and especially of the rectus superior.

2. *On the means of focal adaptation of the Eye.*—The power by which the focal adjustment of the eye is effected has been variously ascribed to the muscles of the eyeball, to the blood-vessels, to the ciliary processes, and to the crystalline lens. The following is an attempt to show that it is not improbable that the effect may be due to the combined action of all these parts, though there is reason to believe that no one of them singly would be a sufficient cause.

Sir C. Bell and Mr Shaw were of opinion that the veins of the eyeball, like those of the brain, are so disposed as to obviate to the greatest extent the effect of any sudden check to the return of blood through the venous system, which might otherwise occasion a free regurgitation that would prove injurious to such delicate and highly-organized structures. Mr Hunt conceives that the *vasa vorticosa* may have some reference to the absorption of the *pigmentum nigrum*. By sending any blood which was repelled in the main trunk into a hundred different channels, injurious pressure of any one part may be averted; but, instead of being arranged, as the veins which empty themselves into the longitudinal sinus are, opening at an acute angle from behind, so as to offer as great an obstacle as possible to the sudden re-entrance of regurgitating blood, the secondary branches of each *vas vorticosum* sweep off from the circumference of the main trunk, course on for some distance without division, and terminate by dividing into exceedingly minute venous twigs. Any blood reaching the main vessels would find, judging from this arrangement, no obstacle to its passage through the secondary vessels. The *vasa vorticosa* leave the choroid and pierce the sclerotic coat at the greatest circumference of the eyeball, passing through the latter tunic obliquely. Hence, when the recti, by their contraction, compress the eyeball, they will prevent the passage of blood through the veins. The blood, thus prevented from returning, will distend the *vasa vorticosa*, the entire venous layer of the choroid and the ciliary processes. Fluid pressing equally in all directions, and being itself almost incompressible, the contents of the eyeball cannot yield to the compression, but are the passive media of communicating it to the most yielding part of the case of the eye, which, as every other part is supported by the tendinous expansions and fleshy fibres of the muscles in action, will be the cornea. This part of the eyeball cannot be pressed from within, without the pushing forward of the ciliary processes, and as, from the adhesion of these to the capsule of the lens, they move together, a corresponding advance of the crystalline lens. The former connection of the choroid to the sclerotic at the entrance of the optic nerve, and just behind the centre of vision, and their consequent slighter vascularity, permit these parts to remain as before, and, therefore, if the lens advance at all, the effect is not counterbalanced by a corresponding advance of the most sentient portion of the retina. If the pupil contract at the same time, from the connection of the base of the iris to the ciliary processes, these will be drawn still more forwards, the vessels of the iris distended, and the lens still further removed from the retina.

The eye is thus fitted for viewing near objects, by the increased convexity of the cornea, its refracting power is increased, whilst, by the greater distance of the screen (the retina) from the lenses, the focal distance is made greater. For distant objects, the muscles being voluntarily relaxed, the blood-vessels contract to their usual calibre, the pressure is removed from the contents of the eyeball, the cornea returns to its usual condition, and the focus of the eye is shortened.

In birds, the return of blood being obstructed in a similar way by the muscles of the eye, the vascular marsupium is filled with blood, and must occupy more space than in its collapsed condition. (Rymer Jones.) The eye being always full, some point must yield to afford the required increase of space. Prevented by muscular action from yielding posterior to the ciliary body, the eyeball is specially strengthened here by a circle of osseous plates. The cornea then must be the yielding part; elastic as a bow, the pressure from behind increases its convexity for the moment, whilst its recoil restores it to its usual condition when the pressure is removed. In addition, Crampton's muscular circle may perhaps assist in bending the cornea, and may advance the lens either directly, or by acting through the ciliary processes.

In fishes, the so-called choroid gland distended, by compression of the choroid veins preventing the exit, whilst its special artery continues its supply, of blood, would subserve the same office as the marsupium in birds. By occupying more space, it must in some degree push forward the yielding parts which lie before it; whilst, from its peculiar horse-shoe shape, a portion of retina in the centre remains fixed and unmoved. The quantity of semifluid gelatinous substance found between the sclerotic and choroid in the eye of these fishes, would promote the equal diffusion of increased pressure. During repose of the eye, when the animal makes no voluntary effort, the circulation through the very vascular choroid body and coat being free and uninterrupted, the distance between lens and retina is that fitted for vision at common distances, being less than is required for the perception of very near objects, or for objects seen through a rarer medium.

Holmes Chapel, Cheshire,
May 8, 1844.

ART. VII.—*Case of Carotid Aneurism.* By JAMES DUNCAN, M. D., Surgeon to the Royal Infirmary of Edinburgh.

ELIZABETH HAUGH, aged 30, unmarried. Admitted into Ward No. 1. of the Surgical Hospital, December 19, 1843.

This patient was placed under my care by my friend, Mr Gilchrist of Leith, who has kindly furnished me with the early history of the case.

She was, by her own and her mother's statement, about 30 years of age; but she looked much older, and had the appearance of a person who had led an irregular life. In her early years she had been in domestic service, but for some time previously to her coming under Mr Gilchrist's care, she had been working in a herring-curer's yard,—an employment in which she was much exposed to cold and wet. Like most of those engaged in the same occupation, she considered the use of ardent spirits indispensable, —drank whisky as often as occasion offered, and also, according to Mr Gilchrist, under favourable circumstances, frequently allowed her indulgence to run into excess. As might have been expected, this irregular mode of life told considerably upon her,—her complexion was bloated, her person flabby, and her whole appearance indicated a depraved state of the constitution.

From several circumstances, ascertained by Mr Gilchrist in his examination of the patient, he was led to infer that, some years previously, she had laboured under some form of syphilis, and likewise, that mercury had been exhibited at that time so as to produce salivation.

She was first seen by Mr Gilchrist on the 21st October last, in consequence of an attack of the epidemic fever. The attack was not characterized by more than usual severity, but the subsequent articular and rheumatic pains were exceedingly troublesome.

Subsequently to this she had a bronchitic attack, but of no great severity, as it yielded speedily to a simple opiate tincture.

She was casually seen by Mr Gilchrist on the 7th December, and she then spoke of a small swelling which had been accidentally discovered two or three days previously, below the angle of the jaw on the right side.

The patient herself spoke lightly of the tumour, regarding it merely as an enlarged gland, and Mr Gilchrist being at the time hurried made no examination. On his calling, however, two days afterwards, about some pauper certificate, she informed him that the tumour had become much larger, and that she thought it must soon burst. Mr Gilchrist now examined it, and immediately ascertained the true nature of the case.

The swelling was situated below the angle of the jaw on the right side, and was somewhat elliptical in form. Its longest diameter, the vertical one, measured about two inches, and its shortest, the transverse, an inch and a-half. It was uniformly smooth on its surface, soft, and easily compressible. When firmly grasped, a pulsation synchronous with that at the wrist, and accompanied with a peculiar thrill, was perceptible over the whole mass; but the thrill was more particularly felt towards its posterior part.

The sac could be emptied of its contents by slight pressure, and was rapidly refilled when the pressure was removed.

When pressure was applied in this manner, the patient complained of a disagreeable throbbing in the temples; or, as she described it, the pressure had the effect of "putting the pulse into the head."

Having thus ascertained the disease, Mr Gilchrist explained to the patient the nature of the case, urged her to submit to the operation for ligature of the artery, and tried to impress upon her the danger of delay. To this, after some days' hesitation, she consented, and was sent to the hospital on the 19th of December, fifteen days after the appearance of the tumour, or, I should perhaps rather say, its discovery by the woman.

During the short period which had elapsed between the date when the tumour was first examined by Mr Gilchrist and the patient's admission into the hospital, it had increased considerably. Its measurements at the time of admission were as follows: 4¾ inches vertically; 6 transversely; 4⅓ at base; 3¾ from base towards chin; 16½ in circumference. The tumour occupied the greater part of the right side of the neck from the mastoid process and angle of the jaw to within two fingers' breadth of the clavicle. Anteriorly it extended as far forward as the symphysis of the jaw, consequently the trachea was very much displaced to the left, and on examining the fauces, a distinct bulging could both be seen and felt behind the pillars of the pharynx on the right side.

The tumour was irregular in form, and its most bulging point was immediately below the angle of the jaw. It varied somewhat in consistence in different parts. Anteriorly it was pretty firm, but posteriorly it was soft and somewhat compressible. When grasped, distinct pulsation was felt, and, in fact, the pulsation could be distinctly seen. At a point about the middle of the tumour posteriorly, a distinct thrill was felt, and a loud bruit heard synchronous with the dilatation of the carotid artery. Nowhere else was there so loud a sound, or so perceptible a thrill; but a little way from the point mentioned, especially downwards, both vibration and sound were perceived in a slight degree. By applying pressure with the point of the finger over this spot, so as to push the tumour slightly to the left, the bruit and pulsation in the tumour ceased, but the pulsation of a deep-seated vessel coursing the posterior part of the tumour could still be felt. On relaxing the pressure, the pulsation in the tumour immediately returned.

The patient was in a highly excited state when admitted into the hospital, and this was so much increased by the visit, that for some time she insisted upon leaving the house; and it was with considerable difficulty, and only by representing to her in the strongest light the danger of her situation, that she was induced to remain. The pulse at this time was rapid, about 130, but fell afterwards to 110, and was of pretty good strength; the skin was moist; the tongue loaded; respiration was hurried and noisy, particularly during inspiration, and, when excited, she had occasionally very slight cough; the face was somewhat bloated, the lips rather livid, and the countenance expressive of considerable anxiety.

The chest was carefully examined, and no morbid sound either in the lungs or heart could be detected. From her excited state, and her expressed determination to leave the hospital, it was impossible to hold any consultation until the 21st.

The propriety of ligature of the carotid was then decided upon; but it was advised to keep her for a day or two at perfect rest, to allow the state of excitement to subside before operating. Gentle purgatives and small doses of digitalis were administered during the interval. She remained in much the same state until the morning of the 25th, having occasionally slight attacks of dyspnœa, but, on the whole, improving; the respiration becoming less frequent, and the pulse gradually sinking to 90.

At 4 P. M. on the 25th, she had what appeared to be a spasmodic attack of dyspnœa of considerable violence. During the paroxysm, the respirations sank to 8 in the minute; but it lasted only for a minute or two, and the breathing then became tolerably easy.

At 6½ P. M. she had another attack, but of longer duration and greater severity.

At 7 P. M. she had a third, still more severe than any of the others, and this had continued for some minutes before I arrived, having hurried over to the hospital on hearing what had occurred.

I immediately decided upon tracheotomy; but before this could be accomplished, the patient had become comatose; the respirations had sunk to 4 in the minute, and the pulse had become almost imperceptible; the face was purple, the lips livid, the extremities cold, and the body covered by a profuse sweat; the trachea was opened close upon the sternum, the projection of the tumour not admitting of its being done higher with safety, and a large tube introduced. The patient did not evince the slightest indication of consciousness during the operation, which was performed as rapidly as possible. As soon as the tube was introduced, a slight amendment in the symptoms took place. The pulse gradually improved in strength; the respirations gradually increased in frequency, until they became 16 in the minute; the face became florid; the patient began to move a little, opened her eyes, and in about 8 or 10 minutes, started up in great surprise and alarm, which was increased on her attempting to speak, and finding she could not do so.

On explaining what had occurred, she became calm, and expressed a desire to have the operation completed.

Sir G. Ballingall, who arrived immediately before I had opened the trachea, agreed with me as to the propriety of doing so as soon as possible. We had been led to this opinion by the observation that the tumour had increased considerably since morning, and had become exceedingly tense; so much so, that we dreaded its giving way if the operation were delayed until next day,—a delay which otherwise would have been most desirable, to avoid the increased difficulty of performing such an operation with imperfect light.

Assisted by Sir George Ballingall, Professor Miller, and Mr Shand, I commenced the operation by an incision of about 2½ inches in length along the inner margin of the sterno-mastoid. The upper part of the incision through the integuments extended some way upwards upon the lower part of the tumour, and was continued downwards over the clavicle, so as to allow the skin to be drawn more freely aside. The sterno-mastoid was raised, and its sternal and part of its clavicular attachment divided. The sterno-hyoid and thyroid muscles were next raised by means of the forceps and likewise divided, and the sheath of the vessel exposed. There was some little difficulty here in consequence of the displacement of the parts, the artery being drawn outwards by the tumour, and the trachea pushed to the left side, so that they were fully an inch and a quarter removed from each other. The position of the artery, however, being ascertained by the finger, the sheath was carefully opened on its inner side, and the needle, armed with a ligature, passed under the vessel. Having ascertained that nothing but the artery was included, the needle was withdrawn, leaving the ligature under it; the ligature was then secured in the usual manner, and the pulsation in the tumour immediately ceased. The edges of the incision were brought together by two points of interrupted suture. No disturbance whatever followed the obstruction of the circulation through the vessel.

The patient bore the operation without the slightest expression of pain or impatience; the respiration continued perfectly easy, being 16 in the minute, and the pulse, though accelerated, was of tolerable strength.

26th. The patient is still in a very satisfactory condition; respiration natural; pulse 90, and of good strength.

27th. Continues the same as at last report. It was thought well, however, as a precautionary measure, to abstract a few ounces of blood, which was accordingly done. The blood drawn was natural.

28th. The tracheotomy tube was withdrawn to-day at 11 A. M., and, after its removal, she continued to breathe with perfect ease, and almost entirely through the natural passage; no air, except when she spake or coughed, passing through the opening into the trachea.

29th. Respiration continues perfectly easy, and in every other respect the patient is in a satisfactory state.

31st. In much the same state as at last report. The tumour is much diminished in size. The following are its present measurements: 4 inches in length; 4 in breadth; 3½ at base; 12½ in circumference. No pulsation has been felt in the tumour since the operation.

January 7. Up to this time, the thirteenth day from the operation, the patient has continued in as satisfactory a condition as possible. She is becoming ravenous for food, and her diet has been somewhat improved. The expression of anxiety which existed at the time of her admission has given way to one of satisfaction and good humour.

8th January. The patient has had a slight rigor this morning, and on removing the dressings a small quantity of grumous decomposed blood was found to have exuded from the lower part of the wound, which had united through the greater part of its extent. This was believed to have been discharged in consequence of a fit of coughing, with which she had been seized at 3 A. M. The chest was carefully examined, and not the slightest trace of any morbid sound could be detected, either anteriorly or posteriorly.

On examination at the visit, the tumour, which had, as I have already mentioned, diminished very considerably, was found to have again enlarged somewhat, and to have become tense. The integuments over it had become red, hot and painful, and imperfect fluctuation could be detected at its anterior part.

The respiration had likewise become somewhat affected and slightly noisy, but by no means so much so as on her admission into the hospital. The pulse had risen to 100, and the patient had become evidently alarmed. The face was somewhat flushed. Twelve leeches were ordered to be applied to the tumour, and small doses of *Tart. Antimon.*, with *Sulph. Magnes.*, to be administered every two hours. I likewise left instructions with Mr S. to dilate the opening in the trachea, which had not yet quite closed, and reintroduce the tube, if the dyspnœa should return, so as to be at all alarming.

9th, 3 A. M. Had an attack of spasmodic difficulty of breathing, but by no means such as to require the introduction of the tube.

Noon. Breathing very easy; fluctuation in tumour somewhat more distinct. I at this time thought that an incision might be made into the tumour with advantage, but it was thought safer to delay doing so until next day, if circumstances should not demand it earlier.

8½ P. M. Some ounces of decomposed blood were discharged from the mouth, the tumour, at the same time, suddenly collapsing.

It was evident from this that the sac of the aneurism had given way internally, and it was likewise evident that it might be opened externally without fear of hemorrhage, the dread of which had prevented us doing so previously. I accordingly made an incision of about two inches in length through the most projecting part of the tumour, and turned out as much of the broken down coagula as I could, with some purulent matter, which was present in small quantity, in all, a little more than a table-spoonful, with considerable relief to the patient.

I left her in what both Sir George Ballingall, who was present, and myself conceived to be a most satisfactory state; the respiration easy, and the pulse of moderate frequency, and good strength. She was seen by Mr Shand at 12 P. M. She had then been sleeping quietly for an hour, and on waking expressed herself as being perfectly easy.

10th January, 2 A. M. The nurse was alarmed at a sudden change in her respiration. This, from being easy, had become suddenly noisy, and, as was described, croupy. She immediately called Mr Shand, who, in a very short time, was at the bedside, but found the patient dead.

Post mortem examination.—This was conducted somewhat hastily, inasmuch as it was done surreptitiously, the friends having refused permission to inspect the body, and those engaged in the examination being in constant dread of their arrival.

The whole anterior part of the neck, from the base of the skull to the origin of the aorta, including part of the upper lobes of both lungs, were removed and laid aside for more careful examination afterwards.

Chest.—Sections were made through the lungs at different parts, and not the slightest trace of any morbid appearance was discovered. The trachea and bronchial tubes were perfectly healthy, and contained no more than the natural quantity of mucus, and none of the sero-sanguineous fluid which had been discharged by the mouth some hours before her death.

The heart was perfectly healthy.

Head.—The vessels of the brain were slightly congested, but no more than might have been expected from the mode in which the woman died. The brain itself was perfectly healthy.

The parts removed were very carefully dissected by my friend Mr Goodsir, and I have annexed a drawing of them.

The aneurism, as I have already mentioned, though much contracted, was found still to be of very great size, extending from an inch from the base of the skull to little more than an inch from the clavicle. In breadth it measured nearly three inches. The parts were necessarily much displaced laterally, the carotid coursing the outer side of the tumour, and the larynx being pushed to the left. The sac of the aneurism was composed of the surrounding condensed cellular tissue, and was partially filled by decolorized fibrine in considerable masses, but was free from any recent coagulum. All the parts were matted together by lymph, so that it was with considerable difficulty that they were exposed. The recurrent of the left side was traced as far as the lower part of the tumour, but beyond this it could not be followed without destroying the preparation as it passed into it and became intimately incorporated with it. The opening by which the artery communicated with the sac was situated at the bifurcation of the common carotid, and was fully half an inch in length, and occupied the greater part of the circumference of the vessel. Below the opening, as far down as the point where the ligature had been applied, the vessel was empty and somewhat contracted. Above the opening it was very much contracted, and, from about an inch above the aperture, was filled with decolorized coagulum. The ligature had been applied a little more than half an inch from the innominata; it was lying loose, and the ends of the vessel were surrounded by a dense mass of lymph. In the vessel, on the cardiac side of the ligature, there was a small coagulum of the usual form, extending nearly to the innominata. I have already mentioned that, a few hours before the woman's death, the aneurism had given way internally. It was found to have done so into the pharynx. The opening was exceedingly small, scarcely sufficient to admit a very slender probe, and was situated on the right side of the pharynx, on a level with the glottis.

This case is interesting in several points of view. 1. The rapidity of the growth of the aneurism was such as is rarely met with, it having attained the very large size which it presented on the woman's admission into the house in less than three weeks.

The causes which led to the formation of the aneurism are of course unknown ; in fact, but little is known generally, in regard to the causes which operate in producing the diseased state of the vessels which leads to the formation of aneurism.

The cause of the rapid growth of the tumour is more easily explained. It is well known that the rapidity of the progress of an aneurism is much influenced by its situation, by its being a true or false one, and likewise by the size of the aperture leading into the sac. In this case we had all these concurring to favour its rapid increase. 1*st*, Its situation favoured its increase, as it was one amply provided with loose cellular tissue, and not bound down by any tense or very resisting fascia in the first part of its progress at least.

2dly, The aneurism was a false one, all the coats of the vessel having been destroyed.

3dly, The opening was very large in proportion to the size of the vessel.

The next point of interest in the case is the production of asphyxia by the tumour, and the very striking effect which tracheotomy had in relieving this state, even when it had proceeded so far as to have induced a state of complete insensibility.

The asphyxia in this case depended, I believe, not so much upon the pressure of the tumour upon the air passages, as upon the irritation of the recurrents which it produced. The dyspnœa was intermittent; not constant, and I have seen the same effect produced by aneurisms and tumours in other cases, in which they did not press materially upon the air passages ; and, in two such cases, I have seen relief afforded to the asphyxia by tracheotomy, although the opening into the trachea was above the seat of the tumour.

The next point worthy of remark is the occurrence of inflammation of the sac, and the effects which it produced, viz. the return of the dyspnœa, and the giving way of the sac by ulceration into the pharynx.

The occurrence of the inflammation was a circumstance which, at the time of applying the ligature, I looked upon as almost certain to follow ; but subsequently, from the rapidity with which the tumour diminished, I was led to hope that it might not take place.

When the rigor occurred, and, along with that, when the nurse mentioned to me that the dressings had been slightly tinged with blood, I was led rather to apprehend secondary hemorrhage. My mind, however, was speedily relieved on this point, by examining the nature of the discharge which had exuded from the wound, and likewise by observing what had taken place in the sac of the aneurism.

The return of the dyspnœa at this time I cannot help regarding as the consequence rather of irritation of the recurrents, than as caused by pressure from increase in the size of the swelling. The nerve, as may be seen, is imbedded in the tumour, and, besides, the swelling, though considerably increased, by no means equalled what had existed at the time of the woman's admission into the hospital. It is possible that relief might have been afforded, or the result which followed prevented, by more early incision; but we had no reason to apprehend what actually occurred; and, besides, from some little dread in our minds of the occurrence of hæmorrhage, we were by no means certain as to the safety of such a step. All dread of this kind was of course removed by the spontaneous giving way of the sac into the pharynx, and then incision was had recourse to without hesitation, and with some slight relief.

The cause of the woman's death is involved in some little obscurity. It could not possibly be the consequence of internal hæmorrhage. The symptoms were not those following death from hæmorrhage; and if any doubt existed previously, this was completely removed by the *post mortem* inspection. As little could it be the consequence of disease either of the lungs or brain. The former were carefully examined some little time before the woman's death, and, besides, no trace of disease was met with on dissection. Under these circumstances I do not know to what else to ascribe the fatal termination than to spasm of the glottis, and I cannot help thinking that if time had been afforded, the reintroduction of the tube into the trachea might have led to a different result.

The accompanying sketch represents the parts as seen from behind.

a. The point where the ligature was applied.

b. The opening by means of which the vessel communicates with the aneurism.

c. The sac laid open, showing the coagula with which it was in part filled.

p. A bristle passed through the opening leading from sac into pharynx.

s. A portion of the posterior wall of the pharynx removed, exposing the glottis.

d. Par vagum.—The recurrent laryngeal passing into lower part of tumour.

Another case has recently come under my care, requiring ligature of the carotid, and I add an abridgement of it from the journal of the hospital, principally because, in the condition of the vessel at the point where the ligature was applied, it presents a striking contrast to the one I have just related.

The man, who was a sailor, and about 60 years of age, was put under my care by my friend Mr Small, in consequence of extensive phagedenic ulceration of the fauces, following a rheumatic fever with which he had been seized after a voyage to the West Indies.

When the patient was admitted into the hospital, the ulceration was very extensive, occupying chiefly the right side, and extending down the pharynx as far as could be seen. The surface of the ulcer was covered with a greyish slough, and the surrounding mucous membrane was of a dull red colour.

His complexion was of a sickly yellow hue, and altogether his aspect was very much that of a person who was labouring under some malignant disease.

The phagedena was arrested by two applications of the nitric acid, and under the occasional application of the nitrate of silver, everything appeared to be proceeding favourably, the ulcer presented a healthy granulating surface, and it was much diminished in extent. On the morning of the 29th March, the twentieth day from his admission, violent hæmorrhage, to the amount of one pound and a half, took place from the left side of the fauces. This ceased spontaneously, but returned in the evening with still greater violence, one pound and a-half of blood having been lost in little more than twenty minutes. The blood was evidently arterial, and as its flow could be arrested by pressure on the left carotid, that vessel was tied immediately above the *omo-hyoid.* The hemorrhage from the left side never returned after the ligature of the vessel, but, on the fourth day, about half an ounce of blood was lost from the right side. With this exception, every thing went on well until the 7th of April, when the patient had a slight rigor, and, on examining the dressings, they were found slightly tinged with blood,—the wound, which previously looked healthy, had changed its character, having become pale and flabby, and the parts where union had taken place had become disunited; pulse 106; skin temperate; tongue rather dry.

On the 8th, the dressing was again slightly stained, but there could not have been lost above a tea-spoonful in all.

On the 9th, there was no return of the bleeding; in other respects the patient was much in the same state as before.

On the 10th and 11th, there was no return of the bleeding; on the latter day it is stated in the report, that "the patient, though weak, was not in a state to excite any immediate alarm, but that he was annoyed by efforts to expectorate some tenacious mucus which had collected in the trachea." There was little or no cough, and on examination of the anterior parts of the chest, which alone could be examined, no morbid sound was detected. Pulse 96; tongue a little moister.

On the morning of the 12th, at five A. M., the nurse became alarmed by difficulty of breathing, which had come on suddenly, and called the house-surgeon, Mr Shand, who found him labouring under very urgent dyspnœa, but perfectly sensible:—the respiration was described by him as being noisy, and resembling that met with in œdema of the glottis. Under these circumstances he performed tracheotomy, after which the patient revived a little and expectorated some mucus through the tube, but died in a few minutes after the operation.

Post mortem inspection.—The head and chest were carefully examined. In the former no morbid appearance was found; in the latter, there was considerable congestion of both lungs posteriorly, but not to a much greater extent than might have been anticipated in a patient who had been confined to the horizontal position so long as this person had been, in the very debilitated state in which he had been left by long-continued disease, and the hæmorrhage which had demanded ligature of the artery.

The parts concerned in the operation were removed and carefully dissected.

The ends of the artery, which had been divided by the ligature, were found lying separated from each other by about a third of an inch, and surrounded by unhealthy purulent matter, but without the slightest effusion of lymph between them. The distal end was closed, the ligature still adherent, and the canal of the vessel filled with coagulum to the extent of an inch. The cardiac end was found contracted to about half its diameter, but perfectly patent for about an inch downwards, and for the same distance, the internal coat of the vessel was separated from the middle by a thin layer of purulent matter. Below this, again, the vessel was completely blocked up by a dense mass of non-adherent decolorized coagulum, which alone had prevented hæmorrhage from taking place.

I have related this case principally, as I have said, to point out the difference in the state of the vessel from that found in the preceding one.

In the first, the process by means of which an artery, after the application of a ligature, is obliterated, was complete, both as regards the internal coagulum and the effusion of lymph around the ligature and the ends of the vessel. In the second it was far from being so; the distal end of the vessel, it is true, was closed, and the canal completely filled with partially decolorized coagulum, but the lower end, as I have said, was patent; there was little or no effusion of lymph between the two, and the only obstacle to the occurrence of fatal hæmorrhage was the coagulum blocking up the canal about an inch from the orifice. In all probability the slight hemorrhage which had occurred some days before the man's death took place from the cardiac extremity of the artery, the blood escaping between the parietes of the vessel and the coagulum; and I have no doubt that it would have occurred to a much greater extent, had the man lived longer, without a more healthy action in the parts supervening, as the softening of the coagulum had already commenced.

The fatal termination of the case was, I have no doubt, occasioned by the obstruction to the respiration from the tough mucus, which the man, in his weak state, was unable to expectorate.

ART. VIII.—*Report of Cases brought before the Liverpool Pathological Society, Session 1843–44, which were illustrated by Pathological Specimens, Drawings, and Casts.*

I.—1. *Case of Fibrous Tumour on the Inner Surface of the Dura Mater.* By JAMES BRUCE, M. D.

MRS BLYTH, a stout woman, aged 52, was seized with an attack of apoplexy during an altercation with her husband on the 26th June 1842. She lay in a state of profound coma, with stertorous breathing and strong quick pulse. The left side of her body was paralysed and completely destitute of sensation; the pupils were insensible to light; deglutition difficult; and her bowels could not be evacuated either by croton oil or enemata. All the usual remedies were employed without benefit. No change took place in the symptoms, and she died on the fourth day.

On inquiring into the previous history of the case, I learned that she had been twice seized with hemiplegia during the last four years, and on both occasions had speedily recovered the use of her limbs. The last of these attacks had occurred about four months previous to her death. During the intervals she had enjoyed tolerably good health, although suffering at times from severe headach and epileptic fits.

Dissection.—On removing the calvarium, the meningeal vessels appeared turgid. At the most elevated part of the right hemisphere of the brain, an oblong grayish tumour, rather larger than a walnut, was firmly attached by a broad base to the internal surface of the *dura mater*, and imbedded beneath in a cup-shaped cavity in the right hemisphere. The cavity was not hollowed out in the medullary substance of the brain, but was formed by a depression of its surface, and the convolutions on which the tumour rested had a shrunk and attenuated appearance. There was no alteration of structure in the surrounding cerebral substance. The tumour itself was of a fibrous structure; it had contracted no adhesions to the cavity in which it was lodged, but had protruded to a very limited extent through the *dura mater*, and produced a slight absorption of the corresponding portion of the cranium.

2. *Case of Tumours on the Branches of the Spinal Nerves.* By JAMES BRUCE, M. D.

Peter Heron, aged 40, died of œdematous laryngitis. On making a *post mortem* examination of his body, a firm white tumour, the size and shape of a hen's egg, and of fibro-cartilaginous consistence, was found attached to the outer side of the left forearm near the elbow. It was overlapped externally by the *supinator longus* muscle, and internally by the *flexor carpi radialis.* It was intimately adherent to a branch of the radial nerve, the filaments of which appeared to be continued over it. On examining the trunk of the radial nerve, it was found studded with small fusiform ganglia, and similar enlargements were observed in the course of the sciatic, crural, and other branches of the spinal nerves. They were at first supposed to be distinct tumours attached to the neurilema of the nerves, or imbedded in their substance, but on farther dissection, each nervous fibril entering into the ganglia was discovered to be the seat of a separate swelling.

The patient had never suffered any uneasiness from these ganglia. Several tumours had been removed from his back by a surgeon some years before, but, like that in the arm, they had occasioned no inconvenience except from their bulk.

II. *Two Cases of Aneurism of the Abdominal Aorta, with Remarks.*

By J. R. W. Vose, M.D., Physician to the Liverpool Infirmary.

I am induced to submit to the society the following short account of two cases of aneurismal disease within the abdomen, because they exhibit some peculiarities which may, perhaps, prove to be not uninteresting.

Case 1.—A man, aged 32, by occupation a porter, and who affirmed that his habits were temperate, came under my care in May 1843. The countenance was pale and haggard. He complained much of pain at the pit of the stomach, which extended to the spinal column, and was felt to pass thence outwards, in the direction of the scapulæ. At the *scrobiculus cordis* there was a distressing sense of throbbing, and, on applying the hand to the seat of the pulsation, a firm circumscribed tumour was felt at each systole of the ventricles to rise very sensibly towards the surface. When the stethoscope was placed over the swelling, a bellows' murmur was heard; this was also the case at the corresponding region of the back; the murmur was nowhere loud, although sufficiently distinct; the bowels were costive; the digestion imperfect and uneasy; no sickness; urine scanty; no œdema. Until about two years since, this man enjoyed good health. At that time he began to experience occasional pain at the pit of the stomach, and became sensible of a pulsation there. He, however, continued to labour until six months ago, when the pain and pulsation became constant, and a tumour was felt at the epigastrium. There has been much pain in the lower extremities, in both testicles, and across the loins. For the last three months he has been able to lie upon his back only.

For some weeks this patient's state underwent no marked change. But about the beginning of June, three or four days before the termination of the case, he began to suffer intense pain in the left flank, which extended along the course of the ureter, and was scarcely felt in the spermatic cord and testicle of the same side. So extreme was the agony, that it caused him to writhe and moan aloud. On the 4th June, the day of his death, this man's countenance had acquired an ashy hue, he was speechless from exhaustion, and expired gradually and quietly.

Dissection, (24 hours P. M.)—Upon opening the belly the peritoneum on the left side exhibited a widely spread purple tint, which was caused by blood extravasated behind it. There was no displacement of any of the viscera, neither was any tumour perceptible.

On dissecting down to the aorta, a small aneurismal pouch, filled with laminated fibrin, occupied the seat of the cœliac axis; between this and the origin of the emulgent arteries, there was a large aperture in the posterior part of the aorta, opposite the upper lumbar vertebræ. This opened into a sac whose sides were formed of condensed cellular substance, while, at the back part, the principal support was given by the bodies of the lumbar vertebræ, which were deeply and extensively absorbed by pressure, while the intervertebral substance was less absorbed. A very large quantity of blood had been infiltrated into the cellular tissue around the kidneys; this was in a coagulated state. At the foramina through which the upper nerves of the loins issue, there were some fibrinous clots, and the *theca vertebralis* was discoloured in this situation. It would appear that the aneurism having cleaned the sides of the chasm which it had made in the vertebræ, had extended to the outer margin of the *Psoas Magnus* muscle, and had there given way. The edges of the opening in the aorta contained many bony points, and the coats of the artery around, and for a considerable distance above this, were thickened by depositions, corrugated and dilated. Below the aneurism the aorta was tolerably healthy, and the same was the case with the arch. Other viscera sound. No trace of peritonitis.

In the foregoing case there was no difficulty, from the first, in coming to the conclusion that the patient's disease was abdominal aneurism. The form of the tumour in the epigastric region, the character of the impulse which it communicated to the hand, and its being the seat of a bellows' murmur which was synchronous with the contraction of the ventricles, were the principal grounds for forming this opinion. Two points connected with the history of this case deserve a short consideration. The first is the cause of the excruciating pain which the patient experienced along the course of the ureter, in the groin, and in the scrotum, before dissolution. The second is the very gradual manner in which death took place.

The cause of the pain I would venture to refer to some change in the form or situation of the aneurism, perhaps to a partial escape of its contents. This would occasion distension of the different filaments of the sympathetic nerves; and although the nerves of organic life seem scarcely, if at all, sensible to ordinary mechanical stimuli, there are certain affections in which, when they become stretched or extended, as in flatulent colic and tympanites generally, great suffering arises. The very gradual way in which death advanced in this patient, seems referable to the circumstance of the hæmorrhage, upon the rupture of the aneurism, having taken place entirely behind the peritoneum, the dense unyielding structure of which, by opposing itself to a suddenly profuse escape of blood, would prevent the occurrence of that immediately fatal syncope, by which these cases so often terminate.

Case 2.—A car driver, aged 42, who had ruined his constitution by drinking, came under my care towards the end of August last. His leading symptom was pain in the right hypochondriac region, where there was found, upon examination, a tumour of considerable size, which was hard, sensibly prominent at the surface, free from tenderness on pressure, and dull upon percussion. The integuments were in no respect altered in appearance over this swelling, and they moved freely upon it; no pulsation of any kind could be felt at or around the tumour. The patient complained of pain in the right shoulder; there was no appearance of jaundice; but his countenance was exsanguine and sallow, and the nose was deeply stained by engorgement of the sub-cutaneous blood-vessels. The appetite was tolerably good; the bowels regular; much prostration of strength; no ascites; no anasarca. His usual position is to lie upon his right side, with his knees drawn up.

This man states that, upwards of a year since, he began to suffer from severe pain, which first affected the left side below the ribs, and subsequently extended itself to the region of the liver, from which it passed upwards in the direction of the right shoulder. From the period of his admission to the 10th of October, the patient's state underwent no change of moment beyond a gradual declension of strength; on that day, when not apparently worse than he had been for some little time, he became faint, and expired suddenly.

Dissection, (24 hours p. m.)—Before opening the body, it was observed that the tumour which had been so conspicuous at the right hypochondrium, during life, had disappeared; and when the abdominal parietes were divided and thrown back, no undue projection existed in the region of the liver;—on the contrary, that viscus, which was pale, but not otherwise changed, was thinner and more elongated than usual. On pursuing the examination a very large quantity of blood, partly fluid, partly clotted, and amounting probably to several pounds, was found in the peritoneal sac. This hæmorrhage had proceeded from the rupture of a large aneurism in the upper part of the abdomen, and which was so situated, in regard to the diaphragm, as to form two divisions; one of these projected into the left pleural cavity, and was filled with coagula; while the other was thin, being merely crusted by a layer of lymph, and contained no clots. It was at the posterior part of this latter portion that the sac had given way; the opening through which the blood had escaped was of considerable size; this, together with the absence of coagula there, must have led to a very rapid effusion of blood. The division of the aneurismal tumour in which rupture of the coats had taken place, was sufficiently capacious to hold nearly two pints of fluid, and where in contact with the spine, it had caused partial absorption of the bodies of the two uppermost lumbar vertebræ. There was no disease in any other part of the aorta. The heart, the great veins, the liver and the kidneys, were pale and bloodless.

In this case there was an entire absence of any characteristic signs of aneurism; while the debauched habits of the patient, his sallow, haggard countenance, the pain which he experienced in the right shoulder, and the existence of an indurated swelling, wholly devoid of pulsation, in the right hypochondrium, led to a strong suspicion of organic liver disease.

It is well known that aneurisms may be fatal, not only where they have never given rise to obvious symptoms, but even without rupture of the sac; and here we have a case in which there was no evidence of the presence of an aneurism, and strong evidence of a totally different disease.

The pain which was felt in the right shoulder, was probably the result of the pressure of the enlarged artery upon the diaphragm, causing irritation of the phrenic nerve, and which would be extended to the cervical plexus, with which that nerve has intimate connections, and several of whose branches are distributed to the summit of the shoulder.

It has been stated that after death, in this case, there was no longer any appearance of the fulness in the right hypochondrium which had existed during life; and that upon opening the abdomen, no tumour was found in that situation, but that the liver was thinner and more elongated than is usual; it seems probable, therefore, that the swelling which occupied the hepatic region, before death, was occasioned by a protrusion of the liver from the pressure of the aneurism behind it, and that, upon the rupture and consequent collapse of the sac, the liver had subsided to nearly its natural position.

There is a remarkable contrast between the slow dissolution of the first patient, and the rapidity with which death took place in this case, where the large size of the rupture into the sac, its taking place in a situation where there were few or no coagula, and the blood bursting into the general peritoneal cavity, instead of gradually insinuating itself behind the serous membrane, are all circumstances highly favourable to the occurrence of a rapid and profuse hæmorrhage.

3. *Case of Aneurism of a Branch of the Splenic Artery.* By Ed. Parker, Esq.

A man was admitted into the Northern Hospital with hæmorrhage from the anus, and died in about an hour a-half afterwards. He was engaged in his usual work at the period of the attack, and his wife mentioned that his health had been always pretty good, with the exception of occasional complaint of slight dyspeptic symptoms and pain of the back.

Autopsy.—A tumour, the size of a small orange, was found situated between, and firmly attached to the great curvature of the stomach, and the transverse arch of the colon. The tumour protruded into the stomach, forming a projection, the size of half an egg. The colon was filled with blood, which had escaped through an opening in the tumour, about three-fourths of an inch long. On further dissection, the tumour was found to be an aneurism filled with coagula, and communicating with a branch of the splenic artery; the trunk of which had divided into two branches a couple of inches before entering the fissure of the spleen. No other morbid appearances were observed.

III.—1. *Disease of Aorta, with Regurgitation through the Aortic Valves.* By Thomas Inman, M. B.

John Keeley, aged 32, admitted January 4, seaman, married, of temperate habits, states that he was always a healthy man, till two years ago, when he was more than ordinarily exposed to wet and cold at sea. This produced cough, expectoration, with pain at the sternum, and between the shoulders. Six months afterwards he began to suffer from palpitation of the heart, which has continued more or less up to the present time. He has at intervals spit up a good deal of bright-red frothy blood. Has been able to follow his ordinary duties as a sailor till about six weeks since. During the last three his legs have begun to swell, an occurrence he never noticed previously.

At present he can move about with difficulty, as motion brings on severe dyspnœa; his complexion is very pallid, and there is a look of great anxiety about his face; he complains much of difficulty of breathing, which comes on in paroxysms; sleeps badly; appetite variable; no faintings or vertigo; pulse regular, 90, with an undulating feel. On placing the hand over any part in front of the chest a purring tremor is very distinctly felt. The arteries of the neck are found to pulsate to a remarkable degree. The dulness on percussion is very extensive over the præcordial region. On applying the ear there is a great heaving impulse, and a loud double whizzing sound is heard; the former prolonged, the latter shorter, and not very distinct at all times. Both sounds are feeble at the apex, but gradually increase in intensity as we approach the base. The diastolic bruit is best heard at this point, becoming inaudible higher up. The systolic appears most intense along the course of the aorta, and is very loud in the arteries of the neck. Lungs resonant, respiratory murmur, marked by mucous rales.

Soon after his admission the dyspnœa became greatly aggravated, and general dropsy came on with frightful rapidity. He was for a time relieved by topical bleeding; but soon after had bloody expectoration, accompanied by distressing orthopnœa. The pulse rose to 100. He was unable to take any rest or food, and died January 18, retaining his senses till the last. Duration of acute symptoms only eight days. *Post mortem* twenty-four hours after death.

On opening the thorax, heart was found immensely enlarged, and all its cavities gorged with blood. The aorta was much dilated. There was some fluid in the pericardium, but no adhesions between it and the heart.

The right ventricle was enlarged equally with the left, the apex being formed by both together. On opening it the tricuspid pulmonary and mitral valves were all found perfectly healthy.

The aortic valves were contracted and somewhat corrugated, quite incapable of closing the orifice. One of them was cribriform, having a large hole in it, which would admit an ordinary-sized quill. The aorta itself was rough with deposits of a consistence between cartilage and the tissue of an artery. The inner coat was, however, entire, and of its usual colour.

The lungs were much gorged with blood, and the bronchi with mucous; at the margin of the lower lobe of the right lung was a small specimen of pulmonary apoplexy.

The liver was congested. The great cavities contained a large quantity of serum. Kidneys and all the other organs healthy.

In this case the following points are worthy of notice:—*Firstly*, The long duration of the disease, without occasioning any urgent symptoms,—showing that a man may have extensive valvular disease, and yet follow a laborious occupation. *Secondly*, The extraordinary rapidity of the symptoms, when they once set in; the duration of the more severe ones being eight days only. *Thirdly*, The accuracy with which the stethoscope effected a diagnosis. *Lastly*, The character of the pulse, and the remarkable throbbing of the larger arteries are only worthy of notice, as being the ordinary concomitants, and almost pathognomonic signs of regurgitation through the aortic valves.

2. *Case of Malformation of the Heart of a Child, which expired on the fifth day after birth.* By James Carson, M. B., Physician to the Northern Hospital, Liverpool.

The child was well developed, and at its birth, and for two or three days afterwards, appeared to be in perfect health, with a natural colour of the skin. The only observation made by its attendants was, that it frequently sighed. It cried well, took the breast, and the digestive functions appeared to be regularly performed;—the bowels were open, and there was nothing unusual in the character of the evacuations. On the third day it was unusually soporose, sighed frequently, and its respiration was more hurried than usual. Its nurse considered it to be inwardly convulsed, which is the popular expression among that class for something going on wrong, which they cannot explain. It ceased on the third day to take the breast, and attention was at that time directed to an unusual dark colour of the skin, which was well described as a duskiness. I saw it on the fourth day. The child appeared to be in a profound sleep, —breathing rather hurriedly, and frequently sighing deeply. There was no fever, and there was no diminution of temperature attracting attention. The ear applied to the chest distinguished a rather hurried action of the heart, but the succession of the sounds was regular. The respiratory murmur was puerile, with a moist crepitus behind. The colour was of a dusky hue, resembling that of a dark-complexioned person. This discoloration prevailed over the body. The obvious conclusion was, that there was congestion of the lungs, and the absence of fever precluded the notion of either pneumonia or bronchitis,—consequently some malformation of the heart was suspected. The child died on the fifth day.

Opportunity was afforded of having a mere peep at the lungs, and of removing the heart.

There was a slight effusion in the pericardium. It is to be regretted that the whole of the contents of the chest were not taken out together, as, from the sequel, it would appear that there was considerable deviation from the natural condition of the blood-vessels, both arteries and veins. There was only one large artery given off from the heart, and from this the pulmonary artery sprung. The thus united aorta and pulmonary artery was considerably larger than the aorta of so young a child. It proceeded from a large ventricle, which appeared at first sight to constitute the whole of the ventricular portion of the heart. The systemo-pulmonic artery was separated from the ventricle by semilunar valve, and from a very large auricle, by tricuspid valve. Into this auricle entered, by three openings, three pulmonary veins, which it would have been interesting to trace, as, from the small size of the two *venæ cavæ*, it is questionable whether one of these veins was not systemic. The large auricle communicated by a small opening with another auricle, of about the size of a small horse bean, into which entered the two *venæ cavæ*, each not above two lines in diameter. This small auricle communicated with a cavity of the size of a swan-shot, in a fatty muscular mass upon the side of the large ventricle, constituting with it the whole ventricular mass, and evidently being the representative of the right ventricle. This right ventricle and the right auricle were separated from each other by small cobweb-like membranes, representing the auriculo-ventricular valve.

The circulation in this, in all other respects apparently well developed child, was thus reduced to that of a reptile of the lowest order, very nearly resembling that of a frog. The very small size of the *venæ cavæ* would lead to the supposition, that a circulation of blood, enabling a child to live in the air for five days, with so slight derangement for two days at least, could not be carried on with the adequate return of blood from the system which such veins would indicate, and that one of the three orifices by which the blood returned into the large auricle was that of a systemic vein. The auricle is of much greater capacity than that of the united auricles of a child of that age ought to be. It cannot even be conceived that extra-uterine life could continue with such an inadequate return of venous blood, as is indicated by the small proportion of the two *venæ cavæ* to the pulmonary systemic artery.

IV.—1. *Case of Strumous Abscess in the right Thoracic Cavity, with external Fistulous opening.* By C. G. Watson, M. D.

E. J., a very delicate looking boy, aged 8, whose father, brothers, and sisters were all sufferers from the effects of scrofulous ulceration of the cervico-facial glands, sought medical advice on account of the following symptoms:—There was a constant cough, with slight expectoration, difficult breathing, night sweats, and progressive emaciation; also pain in the right side, when, in addition to some fulness of lower intercostal spaces, a small ulcerated opening had spontaneously appeared in the space between the eighth and ninth rib at their greatest angle. These had been preceded by two abscesses, one on the right side, which had burst, and discharged matter, leaving the fistulous opening; the other in the axilla of the same side. The acts of inspiration and expiration produced a fizzing noise; the latter especially caused a small extrusion of fluid purulent matter mixed with air, forming large globules, which, in their escape through the opening of about the size of a quill, gave a sound like the bursting of soap bubbles. At first it seemed probable that the opening communicated with the bronchi and lungs; but on further observation it was found evidently not to be the case, and that the phenomena were due to the play of the lungs in the thoracic cavity. Very little purulent matter flowed from the fistula, which was kept ordinarily covered with diachylon plaster.

The stethoscope marked puerile respiration in the left side, and also showed that the upper portions of the right lung were pervious to air; below, the acute pain prevented auscultation and percussion. He died exhausted by hectic, having had two or three epileptic fits towards the close of his sufferings.

Inspection.—A minute autopsy was not allowed. A large abscess occupied the lower part of the right pleural sac, and rested upon the convex surface of the liver, which, with the intervening compressed structure of the diaphragm, bounded it inferiorly. It contained about a pint of fluid, which was of the regular whey-like and flocculent appearance of strumous matter. The eighth, ninth, and tenth ribs were found standing up and bathed in the contents of this strumous cavity. They were denuded of their periosteum, and, though not black, were very easily friable. Their cartilaginous junction with the sternum was entirely gone. The external boundary of the cavity was formed by the skin, serrati, and intercostal muscles. The lungs were not tubercular, as far as examined, but were condensed at the lower part, corresponding to the abscess. Tubercles of the size of peas were, however, found studding the surface of the surrounding muscles and serous membranes. It seems probable that the formation of this large abscess was not due to an inflammation of the pleura, but rather to the maturation of a tuberculous mass,—small possibly, in the first instance, but gradually assuming an extent from its operation as a focus of irritation set up in a scrofulous habit.

2. *Case of Uncircumscribed Gangrene of the Lung.* By G. C. Watson, M. D.

J. L. aged 24, of delicate and spare habit, suffered seven years previously from an hæmoptysis, and about a year since from cough and expectoration, with night sweats and debility. These yielded to treatment, and the intermediate period was without any manifest pectoral affection.

The present attack was ushered in by a febrile paroxysm; the pulse at 130. Calomel and antimonials were ordered. On the second day there was an accession of pain of right side of thorax, with dry hard cough and dyspnœa. The bowels had been freely purged. These symptoms continued in resistance of antiphlogistic treatment until the fourth day, when a sudden and copious expectoration supervened—dark-coloured, fœtid, thin, and similar in appearance to coffee mixed with a frothy mucus. The pulse fell to 100; the tongue was moist and covered with dark sordes; the respiration quick and abdominal; the eyes brilliant; the cheeks flushed; profuse night sweats, and all the other appearances of extreme hectical excitement. It was at this stage of the disease that I joined the usual attendant in consultation. Ascultation found by percussion, clear on left, dull extensively over the right side; the respiratory murmur of the right lung scarcely audible, and, if present, extensively masked by a well-marked moist crepitus. Vocal resonance of right side, generally feeble, except at roots of bronchi posteriorly; respiration of left lung puerile; heart's action not peculiar.

The patient soon afterwards became supine in bed, and unable to recline, even for a few minutes, on the left side, on account of its causing tremendous fits of coughing. No subsequent stethoscopic indications were obtained except gargouillement under the right axilla. The breathing continued laborious; the expectoration, still copious, acquired a greenish colour and thicker consistence, and became more nauseous, and gangrenously fœtid. It was streaked on the eighth day, for a few hours, with bright arterial blood. The breath was fœtid. Neither the solution of the *Chl. Soda* sprinkled in the apartment, nor the inhalation of kreosote, were of any avail in correcting the extreme fœtor of the patient's breath and expectoration. His nurses were unable to remain with him, and even maternal solicitude could not sustain his mother at his bedside.

The further history of the case is a record of orthopnœa, quickly declining strength, profuse sweats, feeble pulse, hippocratic aspect, and other signs of approaching dissolution, until the tenth day, when, in the full possession of his mental faculties, he expired without a struggle.

Inspection.—Configuration of chest normal, save a slight depression of the right and left subclavian regions; percussion clear on left, and universally dull on right side; both lungs adhered to the costal pleura by false membranes, evidently of no recent formation. There was not the slightest appearance of tubercles in the pulmonary tissue of either side. The left lung was normal and healthy; the bronchial tubes in the superior lobe of right lung were expanded sufficiently to admit the fore-finger; their lining membrane was smooth, shining, and of a dark red colour; a large cavity extended throughout one-half of the middle, and almost the entire of the lower lobe. Its sides had a rugged columnar sort of aspect. This appearance was mainly due to the bronchial passages and the blood-vessels having resisted the sloughing and macerating process longer than the cellular tissue; a blood-vessel remained entire, and passed across the cavern from side to side; it was impervious in one-half its course; two or three bronchial tubes floated in the cavity which contained half a pint of greenish thick purulent matter, with comparatively little of the characteristic gangrenous odour, and also some shreds of broken down and macerated lung. The walls of the cavity were only six or eight lines thick, and composed of condensed lung, while the tissue around was gray and crepitating.

In an endeavour which I made to present to the society a tabular illustration of the differential diagnosis between this formidable disease and other heterologous affections of the lungs, compiled from the recorded cases of Laennec, Louis, Graves, Stokes, Carswell, &c. &c., the only sign so constant as to justify the appellation of pathognomonic, is the peculiarly sudden accession of cough, accompanied by a copious fœtid, and thin expectoration.

3. *Sloughing of Vulva, Pleurisy, Gangrene of Lungs.* By F. Inman, M. B. With a Drawing.

Maria Sprounds, aged 31, market-woman; of rather intemperate habits, admitted into the Lock with sloughing of the vulva, perineum, and skin over sacrum. She had at the same time great prostration of strength, hot and dry skin, pulse 120, weak, hurried respiration, (44 in a minute,) with expectoration of a fluid not unlike apricot juice. In consequence of her inability to sit up or to turn over without great suffering, no stethoscopic examination could be made of the chest.

The breath was not fetid, nor had she expectorated any blood.

Various attempts were made to get her history from her own lips, which were unsuccessful; partly owing to the state of debility in which she was, and partly to her always beginning to ramble when she had been speaking long.

The following particulars were learned from her sister. She was always a healthy girl till eighteen months previous, when she began to suffer from what appears to have been bronchitis. She then began to complain of pain in the right side, referred to the region of the liver; which continued more or less till her admission into the Lock. She had the venereal disease six months ago, and was cured.

Three weeks prior to her death, the external part of genitals began to swell: they shortly became black; she went to the Dispensary for a week, at the end of which time the vulva, perineum, and skin over sacrum were in a sloughing state. She then entered the Lock, and, in spite of all remedies, died of exhaustion.

I tried to ascertain the duration of the pulmonary symptoms, but could not obtain any information on this point. The expression of her sister was, that, up to the time of her privates beginning to swell, "she never thought it was much that ailed her; she had always a cough, but nothing to signify, and went about like other folk."

Inspection eighteen hours after death.

Body not emaciated. On opening the thorax, the cavity of the right pleura was found to contain a quantity of turbid serum, the membrane itself being coated with a thick false membrane of recent formation. The costal and pulmonary pleura were adherent at one spot only, where there was great vascularity, and which was found to indicate the situation of a gangrenous cavern.

The left pleura was inflamed, and its cavity contained turbid serum, but not to such an extent as on the other side.

The right lung was carnified at the lower and posterior part by the pressure of the fluid. On cutting into it, at the part where the adhesion and redness indicated internal mischief, three caverns were found lined with a distinct membrane, probably condensed lymph, containing a thin brownish fluid-like serum, mixed with dark-coloured flocculi, and of a very fetid odour. No bronchial tube opened into them as far as could be traced. There were bands intersecting the cavern, like the so-called obliterated blood-vessels in a tubercular vomica, except that the former were black, very ragged, and easily broken down. The lung around them was denser than elsewhere: they were situated laterally, about the centre of the middle lobe.

The left lung was more carnified than the right, as it sank in water. On slicing it, it was found to contain a number of yellowish bodies, which seemed most like very thick purulent matter, apparently contained within a cyst of low organization, but not surrounded with inflamed lung. They occupied the centre of the lung, but were most numerous at the base and margin.

Pericardium slightly inflamed.

Liver was of the ordinary size and colour; but contained three deposits of a pale yellowish homogeneous matter, (the *tuber circumscriptum* of Dr Farre.) At these points the liver was adherent to the parietes by old adhesions, one or two inches in length. They were not surrounded by any distinct cyst, nor was the hepatic substance in the neighbourhood denser than elsewhere. All the other viscera healthy. The gangrene had not invaded any of the parts within the pelvis.

It is greatly to be regretted that in this case no very accurate history could be gathered of the duration and progress of the malady. We had no means of tracing the diseases of the lungs and pleura. From the appearances on examination, it was evident that both were recent; the presumption would therefore be, that they were consecutive of the mortification of the vulva. If we allow this, and consider the latter to be the cause of the former, the occurrence of gangrene in one lung, and of purulent deposits in the other, from the same cause, is very remarkable. In the right lung, as seen in Fig. 2, we have noticed that there were three gangrenous cavities, and only one spot where there was a purulent deposit. In the left lung there was a great number of the latter thickly scattered throughout its substance, and on its exterior, but no specimen of the former.

The absence of fœtor of the breath may be doubted by some, more especially as there was great stench from the sphacelated parts about the perineum; but I am enabled to speak most decidedly upon this point, as I had my face close to hers for a long time, during which I was examining the anterior part of her chest with the stethoscope.

Explanation of Plate.

Fig. 1.—Represents a carnified portion of the left lung, which was covered with recent false membrane. A section has been made to show the size of the deposits. The section through the lung shows their diameter in another direction.

The circle round most of those appearing externally was very distinct.

Fig. 2.—Represents a portion of right lung also carnified. The chief cavity lying under the pleura was marked externally by a bright red tinge, showing its inflammatory character. The costal and pulmonary pleura were adherent at this point.

V.—1. *Abscess of the Liver, containing 13 Pints of Purulent Matter.*

By Thomas Inman, M. B.

Thomas Riley, aged 25, seaman, of dark complexion, states that he is a man of temperate habits, and that he was always perfectly healthy till about ten months since. During the storms of January 1843 he was cast away in the Channel; and afterwards suffered from severe cough, for which he took a quantity of calomel, which made him worse. He went into Cove, where he remained till June, before he was again able to go to sea. Shortly after sailing, he was seized with dysentery, which lasted for six weeks. He got well, left his ship at Trieste, suffered great hardship while there, was taken ill with nausea and sudden debility, which continued for some time, and greatly reduced him. He entered another ship in a very sickly condition, and fell overboard early in this month, and was in the water about half an hour. He was at his work, however, the next day. He subsequently suffered under catarrhal symptoms; applied at the Dreadnought; was told it was a mere cold; sailed again for Liverpool; arrived in a very feeble condition; and was admitted into the Infirmary.

He was then suffering with the ordinary symptoms of bronchitis. This soon gave way to treatment, but left behind a hard dry cough, without much expectoration. About the same time, he noticed that his belly had begun to swell, and he complained of great tenderness in the region of the liver. This viscus appeared to be much enlarged, projecting two or three inches below the ribs. The pain he suffered was constant, at times excruciating.

The swelling continued to increase till it obtained an immense size, appeared to be pointing, and had a distinct fluctuating feel. The tenderness was now confined to one spot, about the size of the palm of one's hand; but there was great irritability of the whole abdominal and thoracic surface, the patient wincing under the slightest touch,—not unlike an hysterical girl. The liver now evidently formed an immense tumour, pushing out the lower ribs on the right side, completely destroying the symmetry of the chest. The abdominal parietes were tense and retracted; there was no ascites. The subcutaneous veins began to enlarge, the body was greatly emaciated, the eyes sunk, the countenance anxious, the pain constant and severe. Decubitus on the left side, with the knees drawn up; lying in any other position always inducing a fit of coughing and severe pain; complains much of thirst; bowels costive; pulse 100; urine deposits lithate sediments copiously. On his admission, he had vomiting occasionally, which soon went off and never returned. He has never had jaundice, or pain in the right shoulder. He continued to suffer greatly from pain till February 15, 1844, during the night of which he seemed faint, and when next seen was dead. There was no cry uttered as indicative of a sudden accession of agony.

Inspection, 24 hours after death.

Body much emaciated; tumour had lost much of its pointed appearance. On opening the abdomen, a quantity of brownish purulent matter escaped, which was collected with care. The parietes of the abdomen were dissected from the anterior surface of the right lobe of the liver, to which they were attached.

The viscus was now seen to occupy a considerable space, the left lobe resting on the spleen, the right extending below to the iliac region, above, to the level of the fourth rib. The right lobe contained a large quantity of fluid, which, on the slightest pressure, passed with great rapidity into the cavity of the peritoneum, through a jagged opening (the size of the first joint of one's middle finger). At this point the liver was not adherent to the parts around. The *margo acutus* also was free; but, with these exceptions, the whole of the right lobe was attached to the parietes by firm layers of false membrane.

The fluid contained within the abscess was a dirty-brown purulent liquid of a fœtid odour, containing here and there some shreds of yellow lymph. That found in the peritoneum was carefully compared with the other, and found to be identical. The quantity was carefully measured, and found to be thirteen pints.

The walls of the abscess were composed partly of lymph and partly of the healthy tissue of the liver, which was, as it were, merely spread out; two abscesses appeared to have existed originally, and to have coalesced. The interior was lined by lymph; at one part assuming the appearance of a brown leathery membrane; at another, looking not unlike tripe.

The *margo acutus* was the only part of the right lobe not implicated; but, on slicing it, it was found to contain a number of small yellowish bodies, like tubercles or concrete purulent matter.

The lesser lobe, the *lobulus caudatus, quadratus,* and *Spigelii,* were all healthy. Weight of the whole liver 5 pounds 1 ounce; gall-bladder filled with healthy bile.

The stomach, spleen, duodenum, small intestines and large, presented no trace of disease, or cicatrices of ulcers; no appearance of general peritonitis.

The lungs were compressed, otherwise healthy. Heart, kidneys, &c. presented the usual normal appearances. Dr Freckleton, in whose practice the case occurred, and with whose permission I have inserted the case, was present at the autopsy.

I may, perhaps, be permitted to remark upon the total absence, in this case, of all the ordinary symptoms which are said to attend suppuration of the liver. There was no pain in the shoulder, although the whole of the right lobe was implicated; no jaundice, though so large a portion of the hepatic substance must have been rendered useless. There was no vomiting from the commencement, if we except a little which he had shortly after his admission —most probably caused by the antimonials he was taking. The abdominal parietes could not be said to be in a constant state of rigidity without pain or tenderness. The cough he suffered from, though sympathetic at last, was certainly bronchitic at first, notwithstanding the absence of these symptoms. The gradual enlargement of the liver as detected by percussion, the form of the abdomen, and the distinct sense of fluctuation, which was the last of all to be observed, made the diagnosis accurate at last, although not till the disease had existed some time. The case is curious, too, as affording an example of what may be termed primary, or idiopathic abscess. I examined the man's history with care, but there was nothing in it to which he could point as the cause; he could recollect no blow or injury on the side; he had not impaired his constitution by the abuse of ardent spirits; he had not met with any injury of the head, or had any symptoms to indicate phlebitis. Dysentery, though a common excitant of suppuration in the liver, cannot have been so in this case, as it had not gone on to ulceration, nor was its duration long enough to have had any influence by extension; besides, when we consider, in this instance, the rapid progress of the disease, when once set in, we can scarcely suppose its first seeds had been sown at least six months before their effect was apparent.

Another point worthy of remark is, the quantity and colour of the fluid. Dr G. Budd, in his Gulstonian Lecture at the College of Physicians, speaks of an abscess of extraordinary volume which contained about two quarts. " A case," he continues, " is recorded by Annesley, in which ninety ounces were contained within the cyst, and one still more extraordinary is mentioned by Portal, in which the matter was estimated at the incredible quantity of twelve pints. These large abscesses seem, in most cases, to be formed by the union of smaller ones."

In the case above detailed the fluid was not estimated; it was measured; some small quantity of course escaped, nevertheless; the quantity actually subjected to examination amounted to thirteen pints (within two ounces), reckoning sixteen ounces to the pint.

From the identity of the fluid in the peritoneum and in the abscess, from the absence of any, even the slightest, appearance of peritonitis, we cannot suppose that the quantity of the purulent matter was increased by secondary serous inflammation.

As regards colour, "the purulent matter from an hepatic abscess is white or yellowish, and precisely like the purulent matter of a phlegmon, and free from odour. In this case, on the contrary, it was of a reddish-brown, and of a fœtid odour. An interesting point of inquiry, and the more so as we cannot solve it, is this, was the rupture of the abscess, and the consequent inundation of the peritoneum, the immediate cause of death, or was it a *post mortem* occurrence, the result of the violence and succussion contingent upon removing the body from the bed to the coffin, and from the coffin to the dissecting table? The latter seems to me the most probable. His death was gradual, with no evidence of sudden access of pain, as if from sudden effusion into the peritoneum; and that violence after death, even though of a trifling nature, will cause rupture of a softened part, whether intestine or the wall of an abscess, I think there can be no reasonable doubt. I have certainly seen one case of dysentery, in which the cæcum was torn across by putting the body heavily upon the table.

2. *Ascites. Cirrhosis.* By T. Inman, M. B.

B. C., female, unmarried, aged 25, brunette complexion, states that, four years ago, she perceived a swelling in the right iliac fossa, which extended upwards, gradually increasing in size till it completely filled the abdomen.

When first seen she was well fleshed, with the skin of the usual colour; the conjunctiva white. The abdomen was immensely distended with fluid, and universally dull on percussion; no enlarged veins were seen coursing under the skin. There was no tenderness in any part of the abdomen, nor, according to her account, had she ever suffered from pain. Catamenia have been irregular from the time the swelling began to be perceived; never had piles; bowels regular; never had jaundice. Her legs were œdematous on admission; soon after this they were attacked by erysipelas, under which she died.

Nothing was ascertained as regarded her habits of life, or whether she was addicted to drinking spirits.

Inspection.—Fluid found to be serous, contained in the cavity of the peritoneum, mixed towards the last with many flocculi of coagulable lymph. The intestines were contracted, and their coats consequently felt very thick; none floated on the surface of the liquid; the mesentery seemed shorter than ordinary.

Part of the jejunum, for about six inches, was in a state of active inflammation. Internally, the *valvulæ conniventes* were enlarged, hard, and coloured with what looked like extravasated blood. The submucous cellular tissue was infiltrated with fibrinous matter, and denser than ordinary. The peritoneal coat was vividly red and its vessels injected, and presenting, at some points, an effusion of coagulable lymph. The mesentery, upon which this part of the intestines was lying, was covered with adventitious membrane of recent formation, and was mottled with purple spots of extravasated blood.

The liver was found shrunk up under the ribs out of sight. It was free from adhesions, and when removed was found to be covered all over in every part with projections of irregular size, forming an admirable specimen of "hob-nailed liver."

The gall-bladder was filled with healthy bile. Water injected into the portal vein entered the hepatic veins freely, and also filled the gall-bladder slowly. The weight of the viscus was two pounds twelve ounces. The spleen was much enlarged, denser than ordinary, and a section presented many dark-coloured spots, as from interstitial hemorrhage; its weight was two pounds and ten ounces.

The ovaries were covered with bright red spots, which projected from their substance, and corresponded with small vesicles internally.

This case is chiefly interesting on account of the great difficulty of the diagnosis. During life, as has been stated, there were no symptoms leading one to infer hepatic disease; on the contrary, the history of its progress, its long duration, the age and otherwise healthy condition of the patient, the irregular state of the catamenia from the commencement, the total dulness on percussion over the abdomen, and the absence of other symptoms, all seemed to indicate ovarian dropsy.

Some of these apparent anomalies were explained by the appearances found after death. The dulness upon percussion all over the abdomen was accounted for by the shortness of the mesentery preventing the intestines floating upon the surface of the liquid.

The absence of enlarged cutaneous veins over the abdomen was accounted for by the circulating system of the liver being in a perfectly pervious condition, as was shown by the rapidity with which water passed from the portal to the hepatic venous system.

The reason why the skin was not of the subjaundiced hue it usually is in cirrhosis, probably was that the secretion of bile was not interfered with to any extent.

VI.—*Cases of Hernia.* Reported by Mr Banner.

1. W. F. aged 53, a mariner, had been subject to a slight scrotal hernia upwards of seventeen years; had occasionally worn a truss, but, from the pain caused by the pressure, had been generally without it. On one other occasion he had difficulty in returning the hernial protrusion, and required the assistance of a surgeon, who immediately reduced it. He had not suffered from the descent for more than twelve months. On the morning of the 24th of March, he awoke at three o'clock, with griping pains and a desire for stool. After straining some time on the night chair, the hernia protruded. He tried for three hours to return it. He then took a large dose of castor oil, and three purgative pills, which were instantly ejected. He vomited but once, and the sickness entirely ceased.

Several other attempts at reduction were made by the patient, and at nine A. M. Mr Macartney was sent for; he applied the taxis, had several large warm water injections thrown into the rectum; some hard fæcal matter came away with the first. At six P. M. he was seen by me, and removed to the Northern Hospital.

The abdomen was free from pain or distension; the countenance calm; pulse 76 and regular; the tongue slightly furred and was moist; he did not suffer thirst or nausea; he had not vomited, or even felt sick since ejecting the castor oil. There was on the right side of the scrotum, a large tumour, which had an irregular surface, and appeared as if composed of three separate swellings. The upper one, the largest, was very hard, and extended through the inguinal canal; the centre one, the most prominent, was elastic, and contained fluid; the lower one, also large, was pulpy and irregular, and was considered by the patient to constitute the testicle, which, he stated, he had much injured some years before by falling from a height across a plank.

Soon after the patient's arrival at the hospital, he was placed in a warm bath of 100 degrees. During the time he was in the bath, blood was abstracted from the arm, and syncope induced. The taxis was again applied without any beneficial result; in two hours afterwards a tobacco injection, of the strength of one drachm to one pint, was thrown into the rectum; in a short time he became faint, and once during the syncope he vomited. A consultation of the surgeons attached to the hospital was held, to consider the propriety of an operation; the majority agreed that the symptoms were not sufficiently indicative of strangulation to justify an operation. This was the more strongly insisted upon, from the absence of the symptom of vomiting. Fomentations were ordered to the tumour, and pills of colocynth and calomel to be taken every four hours.

25th, at 9 o'clock A. M. He had slept at intervals during the night; was free from pain; there was not any swelling of the abdomen. The tongue slightly furred; the pulse 84 or weaker; he had neither vomiting nor felt sick; he had occasionally eructed, had not hiccoughed; the countenance was slightly expressive of anxiety, and he appeared more dispirited; the bowels had not acted, nor had he experienced any inclination for stool; the tumour increased in size, the centre part was more prominent, and appeared to contain a greater quantity of fluid; the penis, which was very visible yesterday, could scarcely be seen, in consequence of the increase of the tumour. He had taken the medicine regularly. The following was prescribed :—

℞. *Ext. Colocynth. Comp.* ℈*ss.*; *Calomelanos*, gr. v.; *Ol. Croton.* gtt. i. *Fiat massa, et divide in pil.* iij. *Sumat* i. *2da quaque hora.*

℞. *Magnesiæ*, ʒiij.; *Aq. puræ*, ℥viii.; *Cap. Coch.* ij. *larga et cochlearium largum succi limonis 2da quaque hora.*

At four P. M. the medicine had been taken regularly; he had not vomited, nor experienced any nausea. The symptoms were as before.

At 9 P. M. there appeared some change in the case; the pulse was 100 and weaker; the countenance a little more anxious; he had hiccoughed three or four times; the tumour was larger; there was neither vomiting nor nausea; the bowels had not acted; the abdomen was more tumefied, though free from pain; he complained of distension. Large injections of warm water and the long tube were ordered to be introduced into the rectum and the following mixture to be taken :—

℞. *Mag. Sulphatis*, ℥i.; *Aq. Menth. v.* ℥viij. *M. Sumat cyathum 2da quaque hora.*

26th, at 9 o'clock A. M.—He had passed a sleepless night; the voice was weak; pulse 110, and weak; hiccough increased; tumour very tense, and painful to the touch; the abdomen more distended; had suffered great thirst, and had drank a considerable quantity of fluid during the night; he had taken his medicine regularly; he had not felt the least nausea, nor had he vomited.

It was now thought that the operation should be performed. An incision was made from the external ring in the course of the tumour; on dividing the spermatic fascia a large portion of omentum was exposed, which passed into the *tunica vaginalis*, and was so adherent to this membrane that it could not be separated; on extending the incision in the *tunica vaginalis* a few ounces of clear serum escaped; two large knuckles of the colon were lying behind the upper part of the omentum, without the external ring; it was very dark coloured and flabby, with lymph deposited on the surface; it did not descend into the *tunica vaginalis*. The omentum was perfectly healthy, whereas the gut appeared much diseased; nevertheless we thought proper to return it. The omentum was left to its adhesions. The edges of the wound were brought together, and the patient placed in bed, after having borne the operation well.

At 5 P. M. He stated that he felt much relieved; the bowels have acted copiously several times since the operation; free from pain; the hiccough continued at intervals; the thirst much less; pulse 120, and weak; surface warm; complains of the want of sleep; the countenance still anxious.

℞. *Calomelanos*, gr. ij.; *Pulv. Opii*, gr. iss. *Ft. pil. statim, et rept. 4ta quaque hora.*

27th. He passed a restless night; has slept at short intervals; hiccough has been troublesome; the pulse very quick and weak; the surface cool; the abdomen tympanitic; the bowels have not acted since last night; he expressed a considerable anxiety to obtain a further action on the bowels; he appeared restless. The long tube was introduced a considerable distance into the rectum, and upwards of a pint of fluid stool escaped, with flatus, which gave great relief; this was, however, of short duration; the feeling of distension, with a sense of suffocation, returned; the tube was again introduced without producing relief; the strength gradually decreased, and during the night he died.

Inspection.—The abdominal parietes were cut through and turned back; the intestines were much distended with flatus, and slightly congested; the colon was drawn down to the inner ring; the greater part of the omentum, much matted and condensed, was without the abdomen; there were a few spots of inflammation on the peritoneal covering of the colon near the ring; the portion of intestine that had been strangulated was lying in the hollow of the sacrum, very dark coloured, and of a dull gangrenous appearance, though tolerably firm to the touch; the mucous membrane lining this portion was of a dark green colour, and easily separated from the middle coat. The intestines contained very little fecal matter, and that was fluid, resembling what had been drawn off by the tube. [The morbid appearances of the scrotum, *tunica vaginalis,* omentum, and testicle, were shown to the society.] There was wanting the proper hernial sac. The omentum was seen descending by the side of the spermatic cord into the *tunica vaginalis;* its first attachment was to the middle portion of that membrane. At this point there appeared a narrowing or constriction of the tunic, leaving an opening by the side of the omentum only large enough to admit the finger; the omentum was continued through this aperture, entering what may be compared to the lower portion of an hour-glass, and adhered firmly at the bottom, and to the testicle, all being covered with a false membrane above the contraction alluded to; the larger portion of omentum very consolidated, was adherent, and formed a large tumour, while the lower part in close contact with the testicle, had given to that organ an irregular shape and peculiar feel perceptible during life. The centre tumour had been formed by the serum contained below the narrowing in the *tunica vaginalis,* and which escaped when the incision was extended through that part.

Remarks.—This case is peculiar, in that it wanted one of the most prominent symptoms of strangulated hernia. Immediately after swallowing three pills and a large dose of castor-oil, the patient vomited once; and in 14 hours afterwards, when syncope was induced by the tobacco enema, he vomited once, and not again, nor did he even feel nausea; the vomiting on both the occasions alluded to may be considered as produced by causes unconnected with the strangulation. Large quantities of fluids and strong medicine had been swallowed without producing nausea or vomiting; it was the want of this symptom, together with the little derangement in the system, that induced two out of the three surgeons to oppose the operation at each consultation.

Another peculiarity in the case was the attachment of the omentum to the *tunica vaginalis* and to the testicle, all of which were covered by a false membrane, which (judging from its perfectly organized state) must have been formed many years.

It is probable that the omentum had become adherent to the testicle before birth, and had descended with that organ. Its adhesion to the *tunica vaginalis* was in two distinct parts; one midway, the other at the bottom; the first so as to form a large tumour of itself; the lower portion, also forming a tumour, was mistaken during life for an enlarged and injured testicle. The rapidly increasing quantity of serum in the *tunica vaginalis,* (so as to project the whole tumour beyond the penis, giving to the whole very much the appearance of a large hydrocele,) are also worthy of note.

All the circumstances of the case taken collectively rendered the diagnosis very difficult; nevertheless it proves, as do many recorded cases, the peculiarities in the symptoms of some herniæ, and of the imperative necessity of operating, when a tumour exists in a situation for hernia, with constipation of the bowels of long standing, more particularly when hiccough constitutes a symptom, which experience generally proves an indication of approaching gangrenous mischief.

Case 2.—William Robinson, aged 32, by trade a butcher, was admitted into the Northern Hospital, under my care, early in October 1843, with symptoms of strangulated hernia.

He had been intemperate ; he had the hoarse voice of an habitual drinker of spirituous liquors, which he stated as natural to him. He had been a subject of inguinal hernia upwards of six years ; but could always, with ease, return the tumour, and had never thought it necessary to wear a truss. The taxis was applied, and some of the contents of the hernial sac returned with a gurgling noise ; the bulk of the tumour, however, remained unreduced. The urgent symptoms quickly subsided, the bowels acted freely, and in ten days he left the hospital in good health, but with a considerable tumour unreduced.

He was re-admitted on the 4th of January 1844, with constipation ; he had vomited on the preceding day, but not since ; he did feel ill ; but had returned in consequence of having been directed to do so in case of vomiting or constipation. The hernial tumour appeared a little larger than when he left the house ; he had griping pain. The bowels had not acted for two days, and were only slightly relieved on the first and second inst.

The pulse was 74 and regular ; tongue a little furred ; the voice retained its husky weakness. The taxis was applied without relief ; a large warm water injection was thrown into the rectum, and some hardened fæces came away ; he was then placed in a warm bath, and, when in a perfectly relaxed state, the taxis was again applied without producing any diminution in the size of the tumour ; he was free from nausea or vomiting ; there was not any pain on pressing the abdomen ; nor had he any pain in the tumour. The injections were continued, and cold applied to the hernia.

January 5. He had passed a good night ; continued free from nausea or vomiting ; pulse quiet ; countenance tranquil ; tongue slightly furred and moist ; bowels constipated.

℞. *Calomelanos*, gr. iij., *statim ; Mag. Sulph.* ʒi. ; *Inf. Sennæ*, ℥i. *Ft. Haust. 4ta quaque hora sumenda.*

He began to vomit as soon as he took the above medicine, which ceased as soon as he discontinued it.

6th. He vomited once in the night, and frequently during the morning ; the matter last ejected was stercoraceous ; he was free from pain in the abdomen, which was slightly tumid. The tumour was painful on being handled ; the skin warm and moist ; tongue furred ; countenance free from anxiety ; voice as usual. The operation was urged ; the patient refused his consent until the afternoon of the 7th instant, (eighteen hours after the operation was first proposed,) during which time the symptoms became rapidly more aggravated ; the countenance anxious ; hiccough ; the pulse quick and weak ; the extremities bedewed with a clammy perspiration ; the abdomen tympanitic ; scrotum dark-coloured and œdematous.

Operation.—On opening the sac a considerable quantity of dark chocolate-coloured fluid escaped. A large piece of omentum, of a similar colour to the fluid, was seen, and adhered to the sac ; under the omentum, and just without the external ring, was a knuckle of small intestine, gangrenous and flaccid. After the stricture had been divided, the protruding portion of gut was gently drawn down, in doing this, it tore away from some recent adhesions at the pubic side of the ring. The intestine was in so bad a state that we determined to lay it open, after this was accomplished, a very small quantity of fæcal matter escaped through the wound. The omentum was left adherent to the sac, very dark-coloured and ill-conditioned. The patient bore the operation well.

At 9 P. M. There had not been any escape of fæcal matter through the wound; the symptoms were much the same as before the operation; the hiccough more frequent; and the abdomen more distended. An elastic gum catheter was introduced a few inches into the intestine without any escape of fæcal matter, it was withdrawn and again introduced, giving it an opposite direction, when several ounces of fæcal fluid flowed away.

8th. He passed an easy night, and appeared relieved, the fæces escaped freely through the wound.

The patient was kept under the influence of opium for two days. This treatment was discontinued in consequence of the delirium which it induced. The gangrenous portion of gut was just separating; the omentum had become mortified. It was the effect on the system caused by this state of the omentum that appeared to produce a sinking, which gradually increased in spite of stimuli and support of every description; low muttering delirium preceded death, which took place on the 15th, eight days after the operation.

Inspection 12 hours after death.—The abdominal parietes were reflected back, and the convex margins of the small intestines appeared slightly congested; there was neither effusion of lymph or serum into the cavity of the peritoneum. Nearly the whole of the omentum had been contained in the hernial sac; the portion within the abdomen was adherent to the inner ring and presented little or no appearance of inflammation. The knuckle of intestine which protruded was formed of about an inch and half of small intestine; it was not the whole circumference of the bowel that had sloughed; the posterior walls of the fold were adherent to the inner ring at the part unoccupied by the omentum, so completely adherent as to prevent any fæcal matter escaping into the peritoneal cavity. The portion of intestine above the protruding part was distended with fæces, whilst the portion below it was contracted and quite empty. The portion of bowel contiguous to that which had descended presented a healthy appearance externally; its lining membrane was slightly congested. On examining the contents of the hernial sac, the gangrenous portion of intestine was not quite separated; just above the point of separation were adhesions to the ring; it was not the whole circumference of the gut that had been destroyed, but merely the upper surface having the canal continuous. The omentum was adherent to the bottom and to the middle of the sac; these adhesions were not of long standing; it was in a complete state of mortification.

Remarks.—In the foregoing case the omentum was dark-coloured and ill-conditioned at the time of the operation, and never in the least recovered itself. In the case of W. F. the omentum was healthy, not even congested. There were adhesions of the omentum to the sac in both cases; in that of W. F. a free anastomosis of the vessels of the *tunica vaginalis* with those of the omentum had probably prevented the same unhealthy appearances here as in Robinson's case, where the adhesions were much more recent, no doubt occurring after his admission into the hospital; they were not organised, and thus may we account for the difference in the appearances of these cases.

It is to be regretted that the omentum was not removed in Robinson's case, as the result would, in all probability, have been different; but the very weak state of the patient, and the doubt as to the exact condition of the omentum, led us to determine to leave it.

3. *Case of Ventral Hernia.* By Jos. King.

Ellen Birmingham, aged 21, *primipara*, of rather slight conformation and lax fibre, but has enjoyed good health.

On her admission, the abdomen was found peculiarly prominent above the umbilicus, and here were the silver lines, instead of being, as usually, at the lower part of the abdomen. The tumour appeared to be rather as an addition to the gravid uterus than to form any integral part of it, and simulated enlargement of the liver.

Her labour proceeded naturally, and terminated in about 18 hours; after delivery the abdominal parietes appeared peculiarly flaccid, especially above the umbilicus; but on bringing the recti muscles into action, the tumour became more defined, and extended between the umbilicus and ensiform cartilage about 7 inches in length, 3½ inches in breadth, and its most prominent part projected about 2 inches. On percussion the sound was tympanitic, and, on pressing the fingers into it, a deficiency, or at least a dilatation, of the *linea alba* was apparent, and the margins were felt tense; she seemed very little inconvenienced by it, and paid very little regard to it. She stated that she had no such swelling before her pregnancy, and that for some months she had not worn her stays, and had been in the habit of turning a mangle. After a cast of the tumour had been taken, she left the hospital, with directions to wear a binder and her stays.

Laurence quotes some cases, the "Eventrations" of the French, reported by Soemmering and Ruysch, in which the relaxation was such that the abdominal tumour has reached to the knees, and the uterus has been completely protruded.

VII.—1. *Ovarian Abscess, containing teeth and growing hairs. Death from ramollissement of brain.* By T. Inman, M. B.

A. S. aged 25, applied to Mr Lewis about eighteen months ago, and by his kindness I am enabled to give the notes he then took of her case. She stated that she had been married for three years, and had had four miscarriages before the fifth month of pregnancy. The catamenia are irregular; appeared copiously a month ago. For the last two months she has been complaining of pain in the right iliac fossa, and shortly after this a small hard tumour was perceptible, painful, and very tender on pressure. After suffering for some weeks, the tumour suddenly became less tender and prominent and she passed at the same time a quantity of purulent matter *per anum*. It afterwards increased, and became, as before, tender, and very prominent; these symptoms were always relieved whenever she had a discharge of purulent matter from the bowels. Her health became impaired; and she had hectic and night sweats. Quina, sulphuric acid, wine, and other tonic remedies were exhibited. The discharge lessened but did not cease; she increased in strength; and, after having been bedridden for months, she left the house, and Mr Lewis lost sight of her for twelve months, till a few days ago, when she came to him complaining of pain in the head, and weakness of the left side, which continued to increase till death, though it never amounted to actual paralysis. The duration of cerebral symptoms was seven days—died comatose, without ever having had convulsions.

Post mortem.—The right ovary was found to be distended into a large sac, which filled the pelvis and was adherent to the rectum, cæcum, uterus, and pelvic fascia, which was thickened and almost cartilaginous. The cyst was found to communicate by a large aperture with the cæcum, by a smaller with the rectum. The cyst, where unattached, was three or four lines in thickness. Internally, it was rough, and not covered with a distinct membrane except at one part. At this part there was a projection not unlike a nailless thumb, covered with a brownish membrane analogous to mucous membrane. This projection was surrounded by a depression, (like an exaggerated *papilla* at the root of the tongue,) of a blackish colour, on the margin of which were growing several (say thirty) hairs, about an inch and a quarter long, having distinct bulbs; their colour was mostly light red. At the top of the projection were three distinct cysts, containing a clear limpid fluid. The contents of the cyst were a dense purulent liquid, in which, (lying perfectly unattached,) three well-formed teeth were found, about the size of the milk teeth when shed; one consisted of two crowns, with the fangs merged into one, giving it a conical appearance; another was curved at the end of the fang like a hunting horn; they contained a central cavity, which was quite empty. It was evident they had never been grown in the usual situation; no place could be found in the cyst to which we could attribute the development of these bodies. On examining the brain, the *hippocampus major* and posterior pillar of the fornix were found in a softened state, which had been the cause of death.

2. *Case of Urinary Calculi containing Hairs.* (*Gravelle Pileuse.*)

By Edward Parker, Esq.

Elizabeth Galley, aged 33, nurse in the Northern Hospital, states that her health has been more or less impaired during the last ten years, and attributes the commencement of her illness to a fall down stairs, by which she supposes her back was injured, although at the time it occasioned little inconvenience. Shortly after the accident, she began to suffer from pain in the loins extending to the left side, and in a few months she experienced pain and difficulty in making water, with frequent desire to void it. The urine after standing awhile deposited a slimy matter, and on several occasions she observed it to be mixed with blood. These symptoms have been greatly aggravated at times, and when suffering most severely the pain is referred to the left kidney, the right being seldom and but slightly affected. Attacks of this nature are most frequent in hot weather; any thing which causes free perspiration, by diminishing the quantity of the urine, produces an aggravation of the symptoms. Ever since the fall, a week is seldom passed without one or more stones being voided, and during the last two or three years she has suffered much from dyspepsia.

The calculi are irregular in form, and vary in size from a mustard seed to a large pea; they are perforated at one end by a minute hair, which is found, when examined with a microscope, to be similar in structure to a common hair, but more slender and delicate. The urine and calculi have been examined by Dr Brett, honorary chemist to the Society; the former is an ordinary kind of phosphatic urine of a pale colour, becoming ammoniacal, and yielding a deposit on standing of the ammoniaco-magnesian or triple phosphate. The calculi are chiefly composed of the same salts.

The circumstance of many of the calculi in the above case being perforated by a hair, appears to me to invest it with considerable interest, and although several cases of a somewhat similar nature have been previously recorded, I presume they are not frequently met with. Magendie, in his work on gravel, notices three, with the exception of which I do not know that any others have been published. The subject of hairs and hair-like bodies in connection with the urine and urinary deposits, is briefly attended to by Dr Prout in the last edition of his excellent work, and also by Dr Brett in the 17th Vol. of the Medical Gazette. In Magendie's cases, the hairs appear to have been very numerous, whilst, in the case I have now related, scarcely any of the calculi present more than a single hair. The origin of these hairs, so far as I am aware, has not been explained. Dr Prout, indeed, mentions that, "in many instances they have been derived from an external source," but he admits "that it is difficult to account for them always in this manner." From observing that, in the present case, the free extremity of the hair uniformly presents a bulb, the pointed extremity being constantly imbedded in the calcareous matter, I have been led to imagine that they are formed by the mucous surface of the bladder,—serve as nuclei for the deposition of the phosphates,—and become detached by the weight of the resulting concretion. The obscurity of the subject, and the impossibility of actually witnessing their formation, lead me, however, to offer this explanation with much diffidence,—as a mere suggestion to be proved or refuted by future observations. The following reasons, I conceive, invest it with some probability.

1st, The analogy of structure between the mucous and dermoid surfaces, and the existence of hairs upon the edges of those orifices where these structures inosculated, render it probable that what is a natural appendage to one, may, in certain states of disease, be produced by the other.

2dly, The fact, that hairs have been found attached to the internal surface of ovarian cysts, the lining membrane of which very much resembled in structure and appearance ordinary mucous membrane. (Vide preceding case by Mr Inman.)

3dly, The uniformity with which the bulb is found at the free extremity of the hair, a peculiarity which could only arise from its being attached by its root when it formed the nucleus for the crystallizing salt.

4thly, If the hairs were introduced from without the whole of their surface would be more or less encrusted with calcareous matter, which would also most probably be the case if they were formed in the pelvis of the kidney.

VIII.—1. *Case of old Dislocation of the Radius and Ulna backwards at the elbow-joint, with new bony cavity in front of the Coronoid process.* By JAMES BRUCE, M. D.

Mrs Brock, a healthy-looking old woman, aged 74, had her left elbow dislocated by a fall in October 1836. The injury was followed by much pain, swelling and stiffness, for which she had recourse to various external remedies; but she did not apply for medical advice till eight weeks after the accident, when her arm had become permanently fixed in a nearly extended position. At this time the limb presented the usual characters of luxation of the radius, and ulna backwards. The arm could not be bent, and the forearm was in the middle state between pronation and supination. There was a protuberance at the bend of the arm, and a slight projection of the radius and ulna at the back part of the elbow, but these appearances were not nearly so well marked as in recent cases of this dislocation. The head of the radius could be felt behind the outer condyle of the humerus. Rotation of the forearm was painful, and accompanied with an obscure motion in the elbow-joint.

Notwithstanding the age of the patient, and the length of time that had elapsed since the accident, it was thought advisable to attempt reduction of the dislocation, as she enjoyed good health, and was anxious to submit to any mode of treatment that might afford her a chance of recovering the use of her arm. On the 9th December, exactly fifty-six days from the receipt of the injury, the usual means were employed for reducing the dislocation. Extension was gradually effected by pulleys, and maintained for some minutes; it was then suddenly relaxed, and whilst an attempt was made to bend the arm across the knee, a snap was heard, followed by immediate flexion of the forearm, which could be bent to an acute angle with the humerus, but on examining the elbow the displaced bones were found in the same position which they had occupied previous to the operation. Another and equally unsuccessful attempt was made at reduction. The patient was then removed to bed with her arm in a sling, and a bandage wetted with evaporating lotion around the joint.

On the following day there was much pain and swelling of the elbow, which required the application of leeches and the constant use of cold lotion. In the course of a week the swelling had greatly subsided, but the pain was very acute on the slightest motion of the joint. On the 1st of January 1837, the patient had a rigor, and the elbow was attacked with erysipelas. There was much constitutional disturbance, with quick pulse and red dry tongue. On the 6th the febrile symptoms had greatly abated, but the elbow was much swollen, and fluctuation could be distinctly felt behind the condyles of the humerus. Nearly four ounces of purulent matter were evacuated by puncture; and for some days the discharge of matter was so abundant as materially to affect her strength, and render stimulants necessary for her support. On the 18th she had a severe attack of influenza, with constant tickling cough and pain in the left side of the chest; the discharge from the elbow assumed an unhealthy character; her strength declined rapidly, and she died on the 22d.

Dissection.—The integuments surrounding the left elbow were greatly thickened and indurated, forming the external walls of a large suppurating cavity, which communicated with the interior of the joint through openings in its ligamentous attachments. The articular extremities of the bones were found to have undergone a considerable change both from compression and the deposition of new bony matter. The lower end of the humerus was thickly encrusted with osseous vegetations, the largest of which, resembling stalactites, projected downwards from its posterior surface as if to meet the olecranon process of the ulna, and fill up the hollow caused by its projection backwards. Plate III. fig. 1. About half an inch below the coronoid process, an irregular-shaped shell of bone arose from the ulna, curving upwards in front of the joint, and partially imbedded in the substance of the *brachialis anticus* muscle. It consisted of a rough plate of bone an inch and a-half in length, varying much in thickness, and presenting several apertures filled with transparent membrane. Its convexity was turned forwards, and its concavity towards the joint, so that it formed a covering to the lower end of the humerus, which occupied the space between its base and the coronoid process of the ulna. The latter process was thrown back into a depression behind the inner condyle, and the trochlear portion of the humerus projected more than usual. The condyles were reduced in size, and denuded of their cartilage. The coronary ligament was partially destroyed by ulceration, but the head of the radius, the sigmoid cavities of the ulna, and the trochlea retained their cartilaginous coverings.

The shell of bone projecting from the ulna was freely moveable at its base, having been fractured by the attempts at reduction; but it was retained in its place by the strong tendinous fibres of the brachialis muscle, which served as a sort of hinge, and admitted of flexion of the forearm to a considerable extent.

The appearances observed in this case are nearly identical with those which are described and figured by Cruveilhier in his great work on Morbid Anatomy, (Liv. ix. pl. 4.) In the case which he describes, the osseous shell arose from both radius and ulna, which were firmly united by ankylosis. The forearm was maintained in a state of flexion, and the slight motion perceptible between the displaced bones and the *os humeri* was occasioned by the latter being disproportioned to the capacity of the new bony cavity. Berard mentions having seen a similar specimen of luxation in the anatomical collection of Beclard.*

With these exceptions I have found no parallel case on record, nor have I discovered any example of this kind of false-joint in our museums of morbid anatomy.

One singular feature in the present case was the facility with which the forearm yielded to the first attempt at reduction. This was at first ascribed to the displaced bones having resumed their natural position, but, when carefully examined, their previous relations were found to have been undisturbed. The real cause of the mobility of the forearm is now explained by the fracture of the new osseous formation, at its point of union with the ulna. The shell of bone was evidently formed in the substance of the *brachialis anticus*. It has been proved by dissection that this muscle is always excessively stretched, and very often lacerated in dislocations of the bones of the forearm backwards; and it seems probable that, in this state, it occasionally undergoes the osseous transformation,—materially aiding nature in her efforts to supply the place of the old articulation.

In dislocations of the elbow the displaced bones are very soon united by ankylosis. In a case of twelve months' standing, related by Mr Lawrence, the radius and ulna were so perfectly ankylosed that, when they were sawn through, they presented no perceptible distinction.† We have no general rule to guide us in determining the exact period at which reduction may be safely accomplished. Mr Liston has been foiled at the end of two weeks, whilst Boyer is said to have succeeded, in a child ten years old, after six weeks. The oldest case of this dislocation which has been successfully reduced is that recorded by Dr Macfarlane of Glasgow. The reduction was effected on the seventy-third day, and, in a month afterwards, the power of moving the joint was in a great measure restored.‡

2. *Description of a diseased Knee-Joint, which was removed by Amputation.* Reported by Ed. Parker, Esq.

In it no trace of a patella could be observed. Some fibrous tissue connected with the tuberosity of the tibia seemed to be the remains of the *ligamentum patellæ.* The external condyle of the femur had completely disappeared. The tibia and fibula rested against that portion of the femur from which the condyle had been removed. The semilunar cartilages were absorbed, and the crucial ligaments partially destroyed. The surfaces of the bones were covered and connected together by a gristly substance, forming a kind of ligamentous ankylosis.

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* Dic. des Sciences Med. deuxieme ed. t. ix.

† Lancet, Vol. ii. p. 434.

‡ Clinical Reports, p. 198.

The appearance of the limb during life was, the knee bent at right angles with the thigh; a slight degree of motion without pain; the head of the tibia dislocated backwards, while the femur projected outwards.

ART. IX.—*On the Circulation in the Acardiac Fœtus.* By G. CALVERT HOLLAND, M. D., Physician Extraordinary to the Sheffield General Infirmary.

CONSIDERABLE attention has of late years been given to the anatomy of the fœtus, and several interesting facts have been established that were previously not known, or only imperfectly understood. The greater exactness of knowledge attained respecting the structure of certain important organs has not, however, thrown any additional light on the functions of them. They are still involved in much obscurity. Indeed, whether the inquiry be the nature of the connection between the fœtus and the mother,—the office of the placenta,—the relations of the umbilical vein and the umbilical arteries in the substance of this organ,—the manner in which fœtal circulation is accomplished,—the uses of the amniotic fluid, or several other points of interest, a remarkable discrepancy of opinion prevails. On all these questions the highest authorities in physiological science are at variance.

On the present occasion, the object is to endeavour to explain the manner in which the circulation is carried on in the acardiac fœtus. The attention has been drawn to this subject, by an article which has just appeared from the pen of a well known writer,* in which it is attempted to be shown, that the circulation in the monster is effected by the heart of the perfect twin-fœtus, which, it is asserted, uniformly co-exists with it. In support of this doctrine, several facts are adduced, which, viewed in the light in which he regards them, seem to afford demonstrative evidence of its soundness. Philosophically examined, they may be shown to arise from a cause very different from what is assigned, nor can they in any degree be employed in corroboration of his peculiar notions. The phenomena which he attempts to trace to the action of the fœtal heart, and on which his reasoning principally rests, so far from having such origin, may be proved to be physical impossibilities. It is difficult to imagine how so enterprising and intelligent an inquirer into the functions of life, and one, indeed, who has devoted a large portion of his time to analytical and experimental investigations into the circulation, should have failed to exercise a sounder discrimination in the appreciation of the phenomena which fall under observation.

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* Marshall Hall, M.D. London and Edinburgh Monthly Journal, No. xxx. p. 541.

A philosopher, to whom science is greatly indebted for many valuable discoveries, remarks, in reference to this subject:—

"It appears that a mola has sometimes been found in the uterus, totally destitute of a heart, in which the blood must have circulated in its usual course through the veins and arteries:—in this case it cannot be ascertained whether there was any alternate pulsation, or whether the blood was carried on in a uniform current, in the same manner as the sap of a vegetable probably circulates. If there was a pulsation, it may have been maintained by a contraction of the artery, much more considerable and slower in its progress than usual; and with the assistance of a spontaneous dilatation; the resistance in the extreme vessels being also probably much smaller than usual; if the motion was continued, it would lead us to imagine that there may be some structure in the placenta capable of assisting in the propulsion of the blood, as there may possibly be some arrangement in the roots of plants by which they are calculated to promote the ascent of the sap."[*]

This acute writer does not appear to have given especial attention to the circulation of blood in the fœtus. He contends, and justly, that the blood must have circulated as under ordinary circumstances in the veins and arteries. The rest of his observations are only ingenious conjectures. Lobstein, in his elaborate work on the nutrition of the fœtus, offers the following explanation. The passage is valuable, as affording a striking illustration of the tendency of the mind to make nature conform to our views, rather than doubt the justness of preconceived conclusions.

"The example of fœtuses void of heart, and which nevertheless grow, by no means proves that the circulation of the infant is continuous with that of the mother. For, in cases in which the heart has been actually wanting, there has always been found some organ which could perform its functions. Thus there has been, at the origin of the aorta, a thickening, a swelling, or a bulb which was instead of a heart; and it was further possible that the large vascular trunks might possess a degree of contractility sufficient to supply the absence of this organ."[†]

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[*] An Introduction to Medical Literature, by Thomas Young, M.D., F.R.S., p. 621.

[†] Essai sur la Nutrition du Fœtus, par J. F. Lobstein, p. 73.

That, in the absence of the heart, the blood is propelled by any modification in the form of the aorta, is entirely a gratuitous assumption. The modification is not constant, and there is no evidence whatever, that any such condition in the higher classes of animals, is accompanied with the power of originating motion.

Another view, which is entertained by some writers, was proposed by Sir Astley Cooper. He imagined the circulation to be inverted, the blood entering the fœtus by the umbilical arteries and returning by the umbilical vein; and, by the further supposition, that an anastomosis exists between the umbilical arteries of the perfect and imperfectly organized fœtus, he endeavoured to show that the heart of the former would maintain the circulation in the system of the latter. He thus expresses himself:—

"In considering the mode in which the imperfect production may have derived its support, the chief difficulty is to explain how its circulation could be maintained without the force of the heart's action, as that organ was totolly absent. This effect has been supposed to arise from the muscular efforts of the vessels themselves. But the difficulty is at once removed, by observing that the imperfect child was a mere appendage to that which was perfect; that there was only one placenta; and the blood which traversed its structure was derived from the perfect fœtus, and returned to it again; that the imperfect fœtus received, through its own umbilical artery, from that of the perfect fœtus, the current, which, after entering the aorta, and thence circulating through the arteries and veins of the monster, was conveyed back again by the umbilical vein of the imperfect, into that of the perfect fœtus; and that the heart of the developed child impelled the blood into the other, which was appended to it, in the same way as it caused that fluid to circulate through the vessels of one of its limbs."*

It has been urged, in opposition to this view, that "it is plain from the injected placenta, or double placentæ, that the umbilical artery is distributed to both portions of this placenta. The force by which it is impelled must therefore be extended, through the capillaries, to the umbilical vein of the imperfect, as well as the perfect fœtus. If the force of the heart is directed, at the same time, into the umbilical artery, it is plain that, being balanced in the two orders of vessels, it will propel the blood along neither; unless some other principle be introduced, to modify this force and direct its influence."†

There are serious objections to this view in explanation of the continuance of circulation in the acardiac fœtus. The blood which circulates, under ordinary circumstances, in the umbilical vein, is not, in appearance or quality, the same as that which returns to the placenta along the umbilical arteries. In the former, it is arterial in colour, and possesses elements essential to support vital action. According to the doctrine of the distinguished surgeon, the blood which has been deprived of these elements, in the system of the perfect fœtus, is alone that which is transmitted to the monster.

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* Guy's Hospital Reports, Vol. i. p. 236.

† Dr Marshall Hall, London and Edinburgh Monthly Journal, No. xxx. p. 543.

This view has not the merit of being suggested by any elaborate or comprehensive considerations of the phenomena of fœtal life. It originates entirely in the imagined difficulty of accounting for the circulation where the heart is absent, assuming, what is certainly destitute of proof, that this organ is necessary to the production of the effect. The motion of the blood, in one class of vessels, is unquestionably facilitated by it in the fœtus; but there is no evidence that the course of the vital fluid along the umbilical vein depends upon any such central power. It will subsequently be shown that this is to be traced to the action of the placental vessels; consequently, if the blood can flow into the acardiac fœtus without the direct co-operation of the maternal or twin-fœtal heart, it is not extraordinary that similar vessels in the monster should maintain the circulation, in a degree commensurate with its wants. To the consideration of this part of the subject we shall hereafter return.

Another view has recently been brought under notice by Dr Houston. He contends that the blood pursues its course to the imperfect fœtus by the umbilical vein, and is conveyed from it, as in the natural state of things, by the umbilical arteries, but that the circulation of it in the fœtus itself is inverted. The following are his remarks:—

"I am of opinion that in monsters of this nature, the placental circulation is normal, viz. that the blood enters the organ by the arteries, and returns by the veins, equally in the imperfect as in the perfect placenta; that there may be, and I suppose there always is, in such cases, a communication between the vessels of the two, which may account for the diminutive size of the one organ as compared with that of the other; but that the presence of such communication is more in favour of the hypothesis, that the currents take the same course in both cords, than of that which infers that they run in opposite directions.

"As to the circulation in the body of the monster, the facts already stated lead me to conclude that the course of the blood in the veins and arteries is abnormal, viz. that the fluid enters by the veins, and returns by the arteries."*

We come, lastly, to the examination of the views of Dr Marshall Hall, who has, for a series of years, given particular attention to the subject of circulation. His intelligence and industry place him in a distinguished position among the physiologists of the day. He attempts to disprove the doctrines to which we have briefly adverted, regarding the circulation in the acardiac fœtus as depending entirely on the heart of the twin fœtus. The following are his observations:—

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* Dublin Journal of Medical Science, Vol. x. p. 214, 215.

"That the action of the heart does extend to the umbilical cord of a second fœtus, is, however, proved by a different kind of fact; the blood of a retained fœtus has been actually seen to flow from the divided cord of the one first born."*

In confirmation of the fact, he mentions a case which fell under the observation of M. Lallemand, the importance of which induces us to give it at considerable length.

"After a labour of some hours, a woman was delivered of a well-made fœtus, which appeared to be at the seventh or eighth month. When the navel string was cut and tied, the end containing the portion attached to the placenta was observed to discharge more blood than usual; a circumstance which made it be examined more carefully. All present were then satisfied that the blood which escaped was discharged by jets to some distance, exactly as an artery of small size would do after amputation. What was the source of this? The blood could not proceed from the mother with that impulse, and those interrupted jets which indicated the influence of the heart. Further, when the fœtus is expelled, the circulation usually ceases in the placenta. We instantly thought then that there was another fœtus in the womb, remembering especially the shape which it had at the commencement of labour; and examination confirmed this conclusion. As the jet of blood was considerable whenever pressure was withdrawn from the cord, it was tied. The contractions of the womb becoming stronger and more frequent, the infant presenting well, delivery terminated naturally. The second fœtus was like the first. After dividing the cord, no more blood escaped by the end connected with the placenta. The delivery of these presented nothing peculiar; the two placentæ were united in one common mass, though the membranes applied were only contiguous. One of the cords was fixed in the centre of the placental mass, the other at its circumference. No attempt was made to inject the placenta, because a portion had been lacerated. But it was evident that there was not only, during the life of the two fœtuses, a communication between them, but further, that this was established by large vessels, since the blood issued from the cut umbilical cord as if it had been only a continuation of the other. This may communicate some idea of the force of the contraction of the heart in a fœtus of about seven months."†

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* London and Edinburgh Monthly Journal, No. xxx. p. 542.

† Observations Pathologiques propres à éclairer plusieurs points de Physiologie, Par F. Lallemand, p. 35, 36, 37.

Dr Hall remarks, "My view of the subject is this: I suppose the heart of the perfect fœtus to impel the blood to the imperfect fœtus, as Dr Young suggested, but not immediately from its umbilical artery into that of the other, according to the view of Sir Astley Cooper, but through the capillaries of the placenta into the umbilical vein and a series of capillaries to the aorta; and not, according to the views of Dr Houston, in an inverted direction."*

It would naturally be inferred, from the concluding remark, that a serious difference of opinion existed between Dr Hall and Dr Houston in regard to the circulation of blood in the body of the imperfect fœtus. The former objects to the views of the latter on the ground, that he advocates an inversion of the circulation; in other words, contends that the blood passes from the umbilical vein into venous capillaries, and is afterwards collected by arterial capillaries and conveyed into the aorta. Dr Hall is compelled to adopt the same doctrine, and, strange enough, it is implied in his own words, as far as any precise meaning can be attached to them. His words are, "I suppose the heart of the perfect fœtus to propel the blood to the imperfect fœtus, through the capillaries of the placenta into the umbilical vein, and through the umbilical vein and a series of capillaries to the aorta." The connection between the aorta and the umbilical vein, in the acardiac fœtus, must depend on capillaries, and which are performing an abnormal function, at least in degree, compared with their office after birth; but in such a case of monstrosity it cannot possibly be otherwise. The absence of a central organ to receive and propel the blood, through two different orders of vessels, renders this condition inevitable. How is blood to get into the umbilical arteries, in its return to the placenta, except by passing through the mass of capillaries which connect the umbilical vein and the aorta? The views of Dr Houston are no more an inversion than those of Dr Hall, nor is there the slightest apparent difference in their views concerning the circulation of the blood in the fœtus. Nature seems scarcely to have left room for a difference on this particular point.

With the absence of the heart, the ordinary connecting links between the venous and arterial systems are destroyed. The *ductus venosus*, the *ductus arteriosus*, and the *foramen ovale*, which, in the fully organized fœtus, establish peculiar and temporary relations to the aortic system, are wanting; and, consequently, unless the two former exist, the aorta must derive its blood from some other channels; and through what other channels, except "a series of capillaries," as stated distinctly by Dr Hall himself? It is indeed singular that this physiologist should object to the views of Dr Houston, on the ground that they are founded on an inversion of the circulation, when his own, as far as they are made known, present no difference.

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* London and Edinburgh Monthly Journal of Medical Science, No. xxx. p. 544, 545.

If large vessels were discovered to join the umbilical vein with the aorta, by which a large portion of the contents of the former were transmitted to the latter, this would not essentially modify the question of circulation in the acardiac fœtus. The physiologist would still have to explain how the blood moves throughout its entire system without the aid of a central propelling organ.

The doctrines of Dr Hall, to be studied with advantage, must be treated under several different heads. It is necessary first to examine the interesting case related by M. Lallemand, as all that is plausible in the reasoning of both physiologists is derived from the consideration of it. It is also important to observe, that the phenomena in this case are not imagined to be evolved by any departure from the ordinary laws of organization; in other words, that they are not peculiar, but universal, in all cases of twins. The flowing of the blood in jerks from the umbilical cord of the child first born, is considered by Lallemand, as well as by Dr Hall, as indisputable evidence of the direct influence of the heart of the second fœtus. The two physiologists differ in one essential respect in their views. They both equally ascribe the ejection of the blood in jerks to the heart; but the former, and by this means he gets quit of one insuperable objection, states that the phenomenon is not only evidence of a communication, during fœtal life, between the twins, "but further, that this was established by large vessels, since the blood issued from the cut umbilical cord as if it had been only a continuation of the other. This may communicate some idea of the force of the contraction of the heart in a fœtus of about seven months."[*]

Dr Hall does not explain the phenomenon on the supposition of an immediate communication between the principal vessels of the twins, but contends that the blood traverses the capillaries of the placenta in its passage to the umbilical vein of the first born fœtus; and thus, as it will hereafter be shown, he adds another difficulty to the one involved in the doctrine, which teaches the direct action of the heart on the motion of the blood in the cord of the twin fœtus. The first step is to analyze this doctrine in its general bearings, and endeavour to point out some of the strange conclusions to which it inevitably leads.

Whether the ejection of the blood from the umbilical vessels be examined according to the doctrines of either physiologist, it is obvious, from the force with which it is emitted, that the communication between these vessels in the divided cord, and the heart of the fœtus *in utero*, is most extensive. The blood that flows along the cord of the child first born must have been transmitted in this course previously to its birth. These authorities state that the communication is natural, and the escape of the vital fluid in a strong current is adduced as evidence of the fact. If such are the relations between the cords after birth, is it not evident that, during fœtal life, each heart is engaged in propelling blood, not only towards its own placenta, and, as might be imagined, facilitating the return of it in the umbilical vein back into the fœtus, but in urging the blood along the umbilical vein of the accompanying fœtus? If this is the direction of the blood after the birth of the first child, it must have been equally so previously. It would be in the highest degree unphilosophical to argue for the one and not to allow the other. Acording to M. Lallemand, the blood flows from the cord as from a divided artery, in case of amputation; and as the impulse of the heart, supposed to be thus exerted, is neither new nor aggravated by any circumstance, its influence must have been equally extensive in every stage of fœtal life.

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[*] Opus cit., p. 37.

It is not easy to see how this objection can be obviated. The doctrine contends for a communication between the two cords, as free as between any artery and vein in the adult body, and a communication that must be as open previous to, as after the birth of either twin. This circumstance is alone sufficient to awaken suspicion of the correctness of the explanation; but this is associated with another difficulty. The blood that escapes from the divided cord is a continuous column with that which is moving in the body of the retained fœtus, and it is a law of the circulating system, that blood will rush towards parts where there is the least resistance. What is to prevent the blood of the remaining fœtus flowing in an impetuous current in the direction of the divided cord? Such is the phenomenon on the division of any artery after birth, and were it to occur previously, death would be the result. Such continuous column is implied in the doctrine; if not, how can the heart of the fœtus expel the blood from the divided cord of the born child? Without such continuous column it is an impossibility; and with it, what is to prevent the blood of the fœtus rushing towards the placenta and the divided cord? In whatever light this doctrine is examined, it is fraught with difficulties. This extensive influence of the heart of the retained fœtus rests on no clearly established facts; nor is it required to account satisfactorily for the character of the stream emitted from the separated funis.

Had not Dr Hall adduced the case of M. Lallemand in confirmation of his views, the communication between the cords of the perfect and imperfect fœtus might have been imagined as peculiar to such remarkable deviations from nature, as an adaptation to meet extraordinary exigencies. But, so far from looking upon it as irregular and characteristic of monstrosity, he regards it as existing in every case of twins, and thereby involves himself in inextricable difficulties. The division of the cord, according to these authorities, is only the occasion which exhibits the communication, and not the occasion which establishes it for the first time.

M. Lallemand, sensible, it would appear, of the difficulty of accounting for the occasional jerks in the ejected stream, had the blood, previously to reaching the cord, circulated through a mass of capillaries, avoids what would have been an additional difficulty annexed to the doctrine, by stating that the communication "avoit lieu par de gros vaisseaux." It is scarcely necessary to remark, that a communication between such vessels is purely a gratuitous assumption.

Agreeably to the views of Dr Hall, it is not by the connection of large vessels that the heart of the one twin transmits an impulse along the cord of the other. He contends that this influence passes through the mass of capillaries intermediate between the two cords. "The difficulty," he observes, "is to conceive that this influence can be so extended through the placental capillaries. A further difficulty would be to conceive that the same influence can extend through a second series of capillaries in the body of the imperfect fœtus, for this is essential to the return of its blood to its placenta,—in a word, to the circulation of the blood."* For the present we will leave the analysis of the circulation in the imperfect fœtus, in order to examine his views respecting the transmission of the blood through the placental capillaries to the cord of the first born child.

There is no instance, in the vast range of animal life, where the blood, issuing from a mass of capillaries and terminating in one large vessel, in the natural conditions of circulation, exhibits, when it is allowed freely to escape, the force and jerking character of a divided artery. The phenomenon is an impossibility, and a little consideration will show the difficulties with which such a doctrine is fraught. It is not imagined that the umbilical vessels exert any particular influence in the projection of the blood. The effect is referred to the fœtal heart alone, and is regarded as the usual and legitimate effect of such a power acting *a-tergo*.

If the heart of the fœtus causes the blood to flow in jerks from the umbilical vessels which bear the relation of veins to it, from the mass of intervening capillaries, how does it happen that the adult heart does not propel the blood through divided veins as through divided arteries? When both classes of vessels are placed precisely in the same conditions, the blood comparatively trickles from the one, while it is ejected from the other with considerable energy, and as long as the heart continues to contract. If the central organ of circulation, in the one case, impresses upon the column of blood derived immediately from capillaries an impulse characteristic of its direct action, the same phenomenon should be observed in the other. In no class of animals, possessing a heart, is the motion of blood, in large vessels receiving the contents of capillaries, at all analogous to that in arteries; nor is is it ever, when permitted freely to escape, ejected with force from them.

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* See Journal, supra cit., p. 544.

No branch of the important subject of circulation has been more elaborately studied than the functions of the capillaries. Though great discrepancy of opinion exists among physiologists concerning the influence they exercise on the motion of the blood, there are, nevertheless, certain facts generally admitted, elucidating the condition of these vessels. The globules, of which the slender streams are composed, have been described, viewed in the field of the microscope, as moving in various directions. The following passages embody the descriptions of several distinguished authorities.

1. Müller, in treating of the motion of the blood in capillaries, remarks,—

"Wedemeyer's description of the course of the blood in the anastomosing capillaries agrees perfectly with what I have observed. 'Sometimes,' he says, 'the red particles flow rapidly from one current into a second, as if by attraction. In other cases the current which they join is very rapid; but they are arrested, as it were, in the collateral current, and only from time to time find means of entering. Sometimes a red particle is even thrown back out of the rapid current into the weaker stream, and is then again repelled. I have also remarked that the same anastomosing branch between two currents sometimes receives the blood in one direction, and sometimes in the other; and that variations of pressure, and position, and motions of the animal, are always the causes of these changes.'"[*]

2. The observations of Adelon are equally pertinent on this subject.

"Lastly, in microscopical observations on living animals, we have seen directly the blood in the small vessels not only circulating from arteries toward veins through the capillary systems, with such phenomena that its progress could not be ascribed to the action of the heart, but often stopping, be, as if hesitating on the direction which it was to follow, and even retrograding with astonishing rapidity, and for a long time. On irritating a white part, the blood is observed all at once to flow into the capillary system of this part, and this system appeared to exert a sort of aspiring or absorbing power on this fluid."[†]

3. The following striking picture of capillary circulation, by Magendie, would scarcely lead us to suppose that the globules

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[*] Elements of Physiology, trans. by W. Baly M. D., Part i. p. 221.

[†] Physiologie de l'Homme, Tom. iii. p. 321.

were urged forward by a strong propulsive power, acting *a-tergo*.

"The irregularities presented by the motion of the globules must then be ascribed to their position, in relation to the adherent column. Thus two globules advance in front with equal rapidity. One of them, impelled by its partner, is driven towards the circumference; its motion is retarded; it remains behind; the other continues its course, and reaches those before. Meanwhile a fresh impulse from another globule gives the retarded one its first position in the centre. Carried in by the current it recovers its original velocity. Previously a globule is placed across the vessel, so that its two extremities are bathed in the motionless column of blood, and its motion is then retarded. Others advance, press it, and accumulate behind. The passage is interrupted; a sort of dam prevents their progressive advancement; the globule, the cause of all the disorder, is shaken, assumes a longitudinal position, resumes its motion, and at the same time all the others resume theirs."*

The description which Dr Hall himself has given of the circulation in arteries, veins, and capillaries, is amply sufficient for our purpose. In treating of the circulation in the web of the frog, he remarks,—

"In the arteries a pulsatory or accelerated movement of the blood is seen quite distinctly,—so distinctly, indeed, as to enable the observer to number the heart's beat with the utmost facility and accuracy. This pulsatory movement is not, in the unimpeded circulation, extended to the blood in the capillaries or veins. By a peculiar anatomy, which will be described shortly, the flow of the blood is made uniform at the very commencement of the former series of vessels."†

In another part of his work he farther observes,—

"In attentively observing the circulation in the web of the frog, we frequently remark anastomosing branches between its veins. In these branches, the course of the globules of blood varies at every moment, and in every possible manner: sometimes this course is from one vein into the other; sometimes it is retrograde, from the second into the first; sometimes it pursues two opposite courses, from various intermediate points, into each of the two veins.

"After observing this phenomenon attentively, we are prepared for detecting other similar phenomena in the capillaries themselves, and especially in those occupying spaces between two adjacent veins. Every kind of movement in the globules of blood may be observed in these vessels, the obvious and distinct result of forces impressed upon them, or of issues afforded them."‡

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* Leçons sur les Phenomenes Physiques de la Vie, Tom. iii. p. 260.

† A Critical and Experimental Essay on the Circulation, p. 26.

‡ Opus cit., p. 86.

In the first passage quoted it is distinctly stated, that, in the unimpeded circulation no pulsatory movement is "extended to the blood in the capillaries or veins;" and, in the latter, the globules are described as moving in all directions, the "result of forces impressed upon them, or of issues afforded them." According to his own views, which are clearly given, the circulation in the capillaries differs as widely as possible from that in the arteries; and that in the veins, into which the capillaries pour their contents, is unlike that in either. The umbilical vessels of the divided cord have the relation of veins to the fœtal heart, and how is it possible for them to exhibit the force and rapidity of circulation belonging exclusively to arteries? If, "by a peculiar anatomy, the flow of the blood is made uniform at the very commencement of the capillaries," how does it subsequently, in the cord of the twin fœtus, regain the jerking character of arterial circulation?

The mass of blood in the placenta, between the vessels of the twin-cords, is many times greater than the quantity sent out by each contraction of the fœtal heart; and, according to the doctrine of this physiologist, every particle of this mass is urged in the direction of the cord of the accompanying or expelled fœtus, and the current which escapes, on the division of it, is stated to exhibit the force and rapidity of arterial circulation. This is a physical impossibility. It might occur, were the blood in the umbilical vessels of the one fœtus moving in continuous and corresponding channels to the vessels of the other, but not otherwise. When these vessels terminate in a mass of capillaries containing many times the quantity of blood which is at any one moment added to, or removed from them, the quantity which escapes cannot possibly exhibit the jerking character or force of that which enters.

In proportion as the mass of blood in the intervening placenta exceeds that which is added to it by each contraction of the fœtal heart, in such ratio will the motion of the mass be less than that in the umbilical arteries, and also, to a certain extent, the current which issues from it. The difference in the capacity of the arterial and venous systems is a just measure of the difference in the rate of circulation in each. Were the communication between the twin-cords to be admitted, the umbilical arteries of the fœtus *in utero* would necessarily have the relation of arteries to the propelling power, and the divided umbilical vessels of the child born, that of veins. It would be an impossibility for the latter, having only the capacity of the former, to remove, in equal times, the quantity of blood conveyed to the placenta by the contractions of the heart. The blood ejected would have a rate of motion corresponding with that of the mass between the two cords, and not with that receiving the direct impulse of the heart and conveyed into the placenta.

Had Dr Hall supposed the communication between the umbilical vessels of the twins to be formed by large and obvious anastomoses, and not in the body of the placenta, by capillaries, the supposition that the influence of the fœtal heart *in utero* extended to the vessels of the divided cord, if not tenable, it would not have been open to the same objections as his own explanation. This, in fact, derives its plausibility from phenomena, which cannot possibly occur on the principles which he lays down. A strong and jerking current, exhibiting the direct impulse of a central propelling organ in the body of the retained fœtus, cannot be observed in divided vessels, separated from this central organ by a mass of capillaries. He admits* that the motion of the blood in the capillaries and veins is rendered uniform by a peculiar anatomy, and yet, in treating of the circulation in the acardiac fœtus, he contends for this jerking current, when every particle of the blood of which it is formed, according to his own ideas, has had to pass through a mass of capillaries to reach the divided vessels of the funis. This is not consistent.

According to his doctrine, the impulse of the heart of the perfect fœtus, transmitted through its umbilical arteries, not only propels the blood in the umbilical vein of the imperfect fœtus, but, by what is called lateral action, draws the blood in its umbilical arteries towards and into the placenta. "Thus, then, in reality, and on two different principles, the blood is propelled to, and attracted from, the imperfect fœtus, by the power and action of the heart of the perfect one, and along the customary channels."† Thus, conjecture is added to conjecture, hypothesis to hypothesis, and conclusion to conclusion, until the whole breaks down, from the superstructure having shot far beyond its narrow foundation. Of the influence of such lateral action there is no evidence, nor does the physiologist possess any knowledge whatever of the nature of the relations between the umbilical arteries of the perfect and those of the imperfect fœtus, so as to show how the blood in the latter vessels is drawn towards and into the placenta by the current flowing in the former. It is greatly to be regretted that a sounder and more cautious spirit is not exercised in the cultivation of physiology. This method of imagining what the mind does not perceive, and endeavouring to prove how nature acts, when her operations do not fall under observation, gives rise to many ingenious speculations, but to few truths of any practical value.

If any apology were necessary for these lengthened remarks, it would be found in the importance of the subject,—in the respectability of the writers whose opinions are analyzed, but especially in the interest which all must feel in the establishment of just principles and correct reasoning. The inquiry is not only valuable in reference to fœtal life, but equally so in regard to vital operations generally. It is scarcely possible to evolve any fact elucidating the conditions of the former, that does not at the same time throw light upon the functions of the latter.

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* See his work, supra cit., p. 26.

† London and Edinburgh Monthly Journal, No. xxx. note p. 545.

In examining the views of distinguished writers, it has been shown that the admission of them leads to conclusions which, if not incompatible with fœtal existence, is attended with serious difficulties; and, further, that the ejection of the blood from the divided cord, which is the principal circumstance on which the doctrines of these writers rest, cannot possibly arise from the action of the fœtal heart. It remains now to explain the cause of the phenomenon, which, indeed, occasionally occurs.

It is remarked by M. Lallemand that the jerking character of the current cannot be ascribed to maternal influence, because on the birth of the fœtus, where there is only one, it is not observed. This is perfectly just; and, moreover, the circumstance may generally be regarded as evidence of the existence of a second fœtus. Without entering minutely into the consideration of the structure of the placenta, there are a few observations respecting the disposition of its vessels to which it is important to direct attention. The organ is composed chiefly of capillaries, but these have very different relations to the arteries and vein of the cord, and which it is essential to point out. The arteries may be viewed as terminating in the placenta, dividing and subdividing until they are lost in its complicated vascular structure; and the blood which they convey is urged forward by a force *a-tergo*. When the cord of the expelled fœtus is divided this force is interrupted. The umbilical vein originates in the placenta, in capillaries, and the vessels gradually increase in size from within outwards, until at length they form the vein of the cord. The circulation in it is to be traced to some power in the placenta. To avoid, in this stage of the inquiry, the expression of any decided opinion on this subject, it may be simply stated, that the influence by which the blood is moved in the umbilical arteries acts in the direction of the placenta,—that which promotes the circulation in the vein, in the direction of the fœtus. Therefore, were pressure exerted on the placenta, it seems probable that the blood would escape more readily from the vein than from the arteries. In the former, to flow out is its natural course; in the latter, to flow out is its inverted action.

After these remarks, the general accuracy of which is unquestionable, it is easy to explain the force and jerking character of the current on the division of the funis. The impulse with which the blood is occasionally ejected, is to be traced entirely to the contractions of the uterus, by which the blood is pressed out in the direction of the vessels of the divided cord. It is almost impossible for such contractions to take place, and not to produce, in some degree, this effect, at least occasionally. The powerful efforts which the uterus makes to expel the second fœtus, and to regain a diminished capacity, is a pressure which is exerted on the mass of the placental vessels; hence the blood will, at times, escape from the divided cord, in jerks, in force and frequency according to the severity and number of these contractions. Why the phenomenon should occur only in cases of twins, is not difficult of explanation. There are several interesting circumstances connected with the condition of the uterus at this time, which do not exist with one fœtus, and which are worthy of particular attention.

1. It is remarked by M. Lallemand, that in cases of single birth, " when the fœtus is expelled, the circulation usually ceases in the placenta." This organ is, at this time, far from being in the same condition as when the birth is double. The vitality of the placenta depends on two things,—the presence of the fœtus, and the undisturbed circulation in the uterus. A change in either circumstance at once modifies its condition. It is not at all necessary, for the broad line of argument which is pursued, that the inquirer should be able to state distinctly the exact nature of the connection between the placenta and the uterus, or the fœtus and the placenta. The generally admitted and indisputable fact, that the placenta receives blood from the fœtus and the uterus, is amply sufficient for the arguments adduced. A severe disturbance in the maternal system, by which the circulation is greatly affected, as by sudden fright, or any distressing mental emotion, frequently causes abortion or miscarriage, and is occasioned by the disorder induced in the circulation between the placenta and the uterus. The death of the fœtus in the womb generally gives rise to the same effect, and from the same cause—disturbance of the circulation in the placenta. This organ is simply the medium of communication between the child and the uterus, and its functions cease with the destruction of this relation. When the fœtus is expelled, and the cord is divided, in case of single birth, it is manifest that the placenta can neither receive blood from the child, nor transmit any to it; hence the mass of placental vessels receiving and circulating the blood conveyed by the umbilical arteries, or urging it into the umbilical vein, in its passage to the fœtus, have no longer any office to perform. Under these circumstances the placenta becomes a foreign body in the uterus, and is quickly expelled. The circulation in it cannot be maintained by its existing connections with the uterus; consequently it would be unreasonable to expect that blood would flow, after the birth of the child, either in quantity or with considerable force from the

divided cord. It does, however, occasionally escape from it, and probably from the umbilical vein, arising from the action of the capillaries in immediate relation with it, and perhaps facilitated by the contractions of the uterus, while its connections with the placenta are not materially interrupted.

2. In case of twins, after the birth of one, the placenta is not in the condition here described. The ordinary relations between the uterus and the placenta, or this organ and a fœtus, are still kept up to a certain extent; and the blood, in diminished quantity, continues to flow from the uterus to the placenta, and from this to the fœtus. There is a remarkable distinction in the condition of the placenta in the two cases. In the one, circulation in it is disturbed and is soon arrested. In the other, it is in its ordinary state, as far as it is possible to be maintained by the remaining twin-fœtus.

3. The continuance of the circulation in the placenta, co-existing with the second fœtus, is the important circumstance to which attention is solicited. Soon after the expulsion of the first child, a certain amount of disturbance is, of course, produced in the placental circulation; and this is quickly followed by the vigorous contractions of the uterus. These contractions are to be viewed as successive acts of pressure, both upon the fœtus and the placenta, and the blood will be forced in all directions, wherever it has an opportunity of escaping from the pressure. It will not be doubted that the numerous capillaries in relation with the cord of the expelled child, will, if the circulation in them is still carried on with any degree of vigour, pour their contents in the direction of the vessels of this cord, and the blood will be ejected in force and in the frequency of successive impulses, in harmony with the energy and number of the contractions of the uterus. In cases of single birth, after the expulsion of the fœtus, the contractions of the uterus are generally exceedingly slight, and, for the most part, seldom take place until the circulation in the placenta has nearly ceased; so that the umbilical cord is placed in very different circumstances, in the two cases. In the one, the ejection of the blood in jerks and in considerable force is occasionally an inevitable effect. In the other such a result would be out of the ordinary course of nature.

Such, according to this analysis, is the cause of the phenomenon, which has been regarded as evidence of the influence of the second fœtus, transmitted to the divided cord of the first born. In the views brought under consideration, have any important data been assumed, as the foundation of them, that are not legitimate and obvious deductions flowing from the physiological conditions, briefly, but clearly specified? If any such objection, however, should be urged, a very slight examination will show that the charge falls with ten times greater force against the writers, the fallacies and imperfections of whose doctrines it is the object of this inquiry to expose.

Whether the blood, which flows from the divided cord, comes from the umbilical arteries or the vein, is a point on which, perhaps, it would be rash to express a decided opinion. It might possibly be ejected from both, but it seems much more philosophical to trace it to the vein than to the arteries. The former is in intimate relation with numerous capillaries, whose office it is to furnish the vital fluid to be conveyed by this vessel into the fœtus, and therefore it is reasonable to regard it as the channel through which the blood escapes.

M. Lallemand adduces another case of twins where one died in the uterus, owing to the loss of blood from the divided cord, and which was found pale and exsanguineous, and this he introduces as conclusive evidence of the justness of his views respecting the influence of the fœtal heart on the umbilical vessels of the expelled child. From the importance which he and others attach to this case, in reference to the present subject, it is imperative to examine its merits. The following are his remarks :—

" If the observation which I have recorded appear not conclusive, I give here another which appears to be completely so :—

" Some days ago a professor of midwifery, in all senses respectable, communicated to his pupils the following fact. (I think I may dispense with mentioning his name, though he related the fact publicly with the most laudable candour.)

" Called to a woman in labour, he observed, after the escape of a first fœtus born alive, that there was a second in the uterus. Being engaged with the infant, he did not examine the portion of the cord attached to the placenta. Very soon, the fœtus left in the womb performed sudden and as if convulsive motions, which the practitioner recognized, his hand being placed on the abdomen. They were so violent, that they caused the mother very painful shocks. But in an instant they ceased all at once. The head had then descended in the cavity of the pelvis. The application of the forceps appeared indicated; this was done speedily and easily. This second fœtus was also as strong and as well made as the first; but it was pale, colourless, quite bloodless; and no assistance could recall it to life."*

This case certainly establishes two facts; the loss of blood from the divided cord, and the death of the fœtus in consequence. The facts will be admitted. It is not, however, shown how they prove the direct influence of the fœtal heart on the blood flowing in the divided cord. The question in dispute is not the flowing of the blood, but the cause of it, which is not in any degree elucidated by this case. It illustrates the prevailing tendency, even in minds of a high order, to jump prematurely at conclusions, rather than analyze the phenomena which fall under observation.

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* Opus supra cit. p. 37.

The condition of the placenta, after the expulsion of one of the twins, has been explained, and it has been stated that the circulation is maintained in it with considerable vigour. The blood, which is supposed to be derived from the uterus, flows in two directions,—towards the fœtus *in utero*, and towards the divided cord. It is more than probable that the vessels of both cords are lost in the same mass of capillaries. Experiments are related in which the double placenta has been thoroughly injected through the vessels of one cord. Vessels may, however, be injected which have no necessary relations to each other as continuous channels, hence such experiments are not free from serious objections.

We do not, on this occasion, presume to express an opinion on the nature of the connection between the ultimate divisions of the vessels of the two cords. The view we are inclined to adopt is, that the umbilical arteries have one termination in common; that is, in the same class of vessels; and that the umbilical veins have also a common origin.

The death of the fœtus, from loss of blood, is regarded as a fact, though only of occasional occurrence; or perhaps, in less exceptionable terms, the death of the fœtus and its exsanguineous condition are phenomena which, at times, fall under observation. We express ourselves in this guarded manner, from the difficulties which involve the consideration of the subject. It is almost needless to remark, that the life of the fœtus *in utero* depends on the regularity of the flow of blood to and from it. To interrupt the supply to it, or to facilitate the escape from it, in a degree disproportionate to the supply, is to endanger or destroy its life. It is possible that one of those effects may take place in conjunction with the flow of blood from the divided cord, and yet this may be only the indirect cause of it. It has been stated, in a foregoing page, that sudden and severe mental emotions frequently cause miscarriages; and it is contended, from the derangement induced in the motion and distribution of the blood, by which the circulation in the uterus, and consequently in its temporary appendages, is disturbed. The escape of the blood from the divided cord may possibly so affect the circulation in the placenta, that the blood of the umbilical arteries of the fœtus may have a tendency to rush into the substance of it, but not out of it, through the divided cord. Or, from the current which escapes, which is probably from the umbilical vein, the blood, which ought to flow through the corresponding vein of the fœtus, may be at rest, or drawn in some other direction. In this case death would result; previously to which, however, a great portion of the vital fluid in the fœtus would have been conveyed into the placenta by the umbilical arteries. It is remarked,—

"Very soon the fœtus left in the womb performed sudden and as if convulsive motions, which the practitioner recognised, his hand being placed on the abdomen. They were so violent that they caused the mother very painful shocks. But in an instant they ceased all at once."* These violent motions of the fœtus would take place on the disturbance of the circulation in it, but this is as easily explained on the supposition that blood has ceased to flow through the vein, as that it has flowed in too great abundance through the umbilical arteries into the placenta.

According to these conjectures, the escape of blood from the divided cord would occasion the death of the fœtus, and this might be found to have lost the greater part of its blood, not because it had been ejected from the severed funis, but because it had accumulated in the placenta. Thus, the loss of blood from the cord would appear to account for the death and exsanguineous condition of the fœtus, when it might possibly be only the predisposing and not the proximate cause of the result.

Let it not be urged that these are only conjectures in opposition to facts. They are offered only as conjectures, not in opposition to, but in the absence of facts. We have often, in the investigation of vital phenomena, felt the justness of the remark, that there are more false facts than false theories. We question not the two facts stated by M. Lallemand, viz. the character of the stream from the divided funis, in the case of twins; or the death and exsanguineous condition of the fœtus, in conjunction with it; and these are the only two facts with which we are favoured.

It is not possible to select from the whole range of physiology a more forcible illustration of gratuitous assumptions than is furnished by distinguished inquirers on this subject. Their views are conjectures without elaborate consideration,—without even casting a thought on the vast field of phenomena between the effect and the imagined cause. From the jerking character of the current, to the influence of the heart of the retained fœtus, there is no investigation to show whether the blood comes from the vein or the arteries, and yet the determination is fraught with important truths. There is no effort made to ascertain whether the phenomenon is possible,—no statement of the difficulties attending every step of the inquiry. We are simply informed that the blood escapes in jerks, and with that rapidity of conception peculiar to genius, it is at once decided that jerks are evidence of the action of a heart, and as there is no such organ in direct connection with the cord which exhibits them, the heart of the retained fœtus, which has its own funis and its own maternal sauguineous relations, is abruptly seized upon to explain the circumstance. And this is made to do extraordinary duties,—to send the blood through the greater part of one fœtal system, through its own cord and placental vessels, and through a mass of capillaries, and yet, after all, to cause the blood to flow from the divided cord of another fœtus, with the same precise character that it would have been ejected from its own arteries!

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* Opus supra cit., p. 37, 38.

Other considerations tend strongly to call in question the justness of the view entertained by M. Lallemand and Dr Hall, respecting the action of the fœtal heart on the divided cord of the first born child. The former writer observes, "Meanwhile, among the authors who have written on midwifery, some speak not of ligature of the two ends of the cord, in cases of compound pregnancy; others regard it as useless; some recommend it only vaguely, without attaching to it any importance."* Did the ejection of the blood from the divided funis arise from the cause assigned, viz. the heart of the retained fœtus, such a phenomenon, in place of being only an occasional occurrence, would be the rule, and its non-appearance the exception. To divide an artery, of the size of the vessels of the cord, in any part of the body, is to produce invariably a strong current in relation with the successive impulses of the heart.

The escape of the blood from the cord is regarded by these writers in the same light, and the attention is directed to it as an effect springing out of an obvious cause,—the contractions of the fœtal heart. Why is not the phenomenon an equally universal occurrence? If the heart, in one case, always expels the blood with force, why does it not in another? This is an important question for those who contend for a similarity in the causes. The occasional occurrence of the phenomenon is an insuperable objection to the doctrine which ascribes it to the action of the fœtal heart. In inquiries of this kind it is not only necessary to point out the assumed agent in the production of an effect, but imperative to show why this agent, which is always in operation, gives rise to partial results. There is not the same weighty objection to the views by which it is proposed to account for the phenomenon. According to these, it depends on the condition of the placenta, and on the energy of the contractions of the uterus, which are circumstances liable to numerous modifications. We will endeavour to point out the nature of some of them. If the birth of the first twin has been easy and by no means abrupt, the circulation in the placenta having a relation to this fœtus, or in that part of the placenta which is more or less common to both, may have assumed a condition preparatory to such change. It may gradually have become enfeebled, as well as the connection between the placenta and a certain portion of the vessels of the uterus less intimate, so that on the expulsion of the child the uterine vessels do not continue to pour into the placenta a large stream of blood, but are much in the same state as when the fœtus is expelled in cases of single birth. If, on the other hand, the labour has been abrupt, or has taken place before the uterine vessels have adapted themselves to an altered state of things, the subsequent contractions of the uterus will, in all probability, cause the blood to flow freely from the divided funis. The phenomenon will depend on the condition of the placental and uterine vessels in connection with this funis.

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* Opus supra cit., p. 38.

The same gradual changes, with respect to the retained fœtus, may be an additional circumstance preventing the ejection of the blood, though we do not profess to understand the influence which it would exercise, except on the supposition that the mass of vessels in the placenta in relation to one cord, may either directly or indirectly anastomose with the corresponding mass in relation to the other. The admission of this offers no very forcible objection to these views, because the diminution in the activity of the uterine circulation, to which we have alluded as an important circumstance, will, of course, lessen the vascularity of the placenta, and proportionately the tendency to the escape of blood.

The views which are here brought under consideration will explain why, occasionally, in cases of single birth, blood escapes freely from the funis in connexion with the placenta. It sometimes happens that the fœtus is expelled before the circulation between the placental and uterine vessels has been gradually and greatly diminished; consequently, the remaining activity in the capillaries of the placenta will cause the blood to flow from the divided cord. The severe contractions of the uterus not only tend to expel the child, but, in an equal degree, to lessen the activity of the uterine circulation, and, according as this is effected, will the subsequent condition of the placenta be modified.

We pass now to the consideration of some other views which have been brought into prominent notice by Dr Hall, and imagined by him to confirm the justness of his arguments, with respect to the circulation in the acardiac fœtus. After adverting to the case of M. Lallemand, he endeavours to show that there is no objection to his own views, on the ground of the extensive influence which he assigns to the heart of the retained fœtus. As already remarked, this influence is conceived to be transmitted, not only through the capillaries of the placenta, but, when the acardiac fœtus is one of the twins, through the entire system of the monster. " But have we not," he says, " examples of such extended influence of the heart? Do we not observe that, in fishes, the blood is distributed first through the capillaries of the lungs, then through the capillaries of the various organs of the body, and, lastly, again through the capillaries of the portal circulation? If any one doubt the extent of this power, it is incumbent on him to show that there is indeed an auxiliary or additional one; and of this I again assert, we have hitherto no proof."

If the inquirer into physical science were allowed to assume his facts, as the physiologist does in this instance, the study of philosophy would call largely into exercise the imaginative powers of the mind. So far from the circulation in fishes being evidence of the justness of his reasoning, we should adduce it as evidence of the co-operation of the capillaries in the process of circulation; and there would be much less assumption in this than in the reasoning of this physiologist. In fishes the heart sends the blood to the gills, as the right ventricle in warm-blooded animals transmits it to the lungs; and, in both cases, for the same purpose,—to acquire vital properties. In the latter instance, it is conveyed into the left ventricle, by the contractions of which it is sent throughout the arterial system. In fishes there is no such corresponding cavity to receive and propel the vital fluid after the chemical changes in the gills; and from the consideration of this circumstance, in conjunction with numerous other phenomena, we contend for the influence of the capillaries in the process of circulation. Why the phenomena should be imagined to confirm the views of Dr Hall, and should be seized upon as setting at rest the question in dispute, viz. the extent of the influence exercised by the heart, we are at a loss to understand.

In some experiments, which he relates, on the eel, he attempts to prove that the influence of the heart is transmitted through a secondary series of capillaries. "We placed," he observes, "the pectoral fin of the eel in the field of the microscope, and compressed it by the weight of a heavy probe. The movements of the blood became most obviously pulsatory, pretty regularly about twenty times in the minute—the precise number of the pulsations of the heart. The heart's action extends, therefore, through the branchial capillary vessels, to the secondary series of capillaries in the body of the animal. We now tied the vessels proceeding from the heart; all circulation in the tail and fins subsided almost immediately, affording a proof that the capillaries have in reality no power of sustaining the circulation. Now, if in any animal the capillary vessels have a power of their own, it is surely in the eel. For this is the longest of animals, and its systematic circulation is entirely secondary, both circumstances requiring such power."

We have, in some previous inquiries,[*] shown the unphilosophical mode of procedure adopted in such experiments, and the sources of fallacy to which they are open; consequently on this occasion our strictures will be brief. Had we not been familiar with this branch of physiology, the consideration of these experiments would have left on the mind an impression that, antecedent

[*] In a series of papers published in the Edinburgh Medical and Surgical Journ.

to his labours, the circulation of blood from arteries through interminable capillaries into veins, had never been clearly established. Indeed, the only value which these experiments possess, is to prove that blood flows from one class of vessels into another. There is, however, one novelty in the conception of them, and that is to show that the capillaries exercise no influence on the motion of the blood, and to establish this he deprives them of the vital stream,—an essential condition to their action. This first experiment consists in placing a heavy probe on the pectoral fin of the eel, for the purpose of arresting the course of the blood at this point, so that between the probe and the heart there would be a series of columns of blood at rest, the vessels in which they are contained assuming the condition of inert tubes. As the blood cannot possibly move forward, it is evident that every additional quantity sent into these vessels by the contractions of the heart, must aggravate the existing tension, and at length this organ acts on these columns, as the piston would act upon a series of inorganic tubes. The physiologist remarks, that he perceives, in the secondary series of capillaries, pulsatory movements, synchronous with the contractions of the heart. It would be strange if he did not. What is the blood to do that is constantly added to the columns by the successive contractions of the heart? The probe prevents them moving forward, so that every subsequent quantity of blood must either return into the heart, or impress upon these columns an impulse which must be perceptible throughout the whole extent of them. If such impulse were not perceptible, it would be because there was no continuous channel between the probe and the heart, the channel existing, the pulsatory movement is inevitable. The phenomenon, to which the attention is directed, might have been calculated upon independently of all experiments. We admit the pulsatory movement, and confess we should have been more astonished at its absence than we are at its presence.

His next experiment is in character with the preceding. It is to show that the capillaries are entirely destitute of all propulsive power, and this he proves in a very original manner. The reader would imagine that in order to determine this important point, the physiologist would give the vessels fair play. If it were proposed to ascertain whether one individual could knock another down, it would not be right to tie his arms behind him. Is it common justice to the capillaries, in the investigation of their functions, to tie the vessels immediately proceeding from the heart, after which the capillaries cannot receive a particle of blood, and then inquire whether they have the power of urging this fluid forward? Such is the experiment to which our attention is solicited, and which is considered to be as conclusive in its results as it is original in its conception.

PART II.

CRITICAL ANALYSIS.

Art. I.—*Guy's Hospital Reports.* Second Series, Nos. 1, 2, and 3, from April 1843 to April 1844. Edited by George H. Barlow, M. D., &c.; Edward Cock, F. R. C. S., &c.; Edmund L. Birkett, M. B., &c.; J. H. Browne, M. B., &c.; and A. Poland, Esq.

The three numbers of the Second Series of Guy's Hospital Reports contain several interesting and valuable papers. Mr Alfred Taylor, who has already published many instructive papers on subjects of medical jurisprudence, has several papers. The first of these is entitled

Case of suspected irritant poisoning, with remarks on the poisonous properties of certain kinds of decayed animal matter used as food.—Three members of the family of a shepherd—the wife, the son, and daughter, the two last being young children, were taken ill on Sunday, December 20. They were better by next day, and dined on mutton with the father on Monday at 11 o'clock. The father did not return till the evening, when he found the boy dead, and his wife and daughter in a state of insensibility. He stated that when at work he felt a sharp burning pain in his inside, for which he could not account. The mother, after her recovery, stated that she felt great pain after the meal, but she could give no further account of her symptoms. It was learned, however, that she foamed at the mouth, and was in a state of great nervous excitement. She suffered slightly from vomiting, but there was no purging. The boy and girl were both affected with vomiting and purging, and the boy must have died in less than three hours, as he was found dead at three o'clock. The motions of the boy were of a dark green colour; the matters vomited were copious in quantity and streaked with a yellow-coloured matter. The matters vomited by the mother were described as having a bright glistening appearance, like quicksilver, on the surface. The contents of the stomach of the mother were drawn off by the stomach pump about eight in the evening, and reserved for chemical examination.

The body of the boy presented the following morbid appearances. The cutaneous surface was unusually pallid. The lungs were loaded with blood of a scarlet colour. The heart was natural; the liver of a pink colour, congested with very liquid blood. The stomach contained a small quantity of half-digested food, and presented on its posterior part several prominent rugæ which were inflamed, while traces of inflammation were present on other parts of the lining membrane. The small intestines were inflamed in their upper portions, but the appearance of inflammation was less marked inferiorly. They contained a liquid mixed with blood. The muscular coat of the rectum was very red, but in other respects there was nothing abnormal either in the large intestines or their contents. The peritoneum was highly inflamed. The bladder was contracted; and on its posterior wall were two spots of well defined inflammation. The spleen and kidneys were healthy. About two ounces of bloody serum were found in the cavity of the abdomen. The upper part of the larynx and lower part of the pharynx were inflamed, and there were also traces of inflammation at the bifurcation of the trachea. The veins of the head and of the brain were congested with blood, but there was no morbid change in the structure of the brain.

The contents of the stomach and bowels were collected and subjected to analysis, but no traces of poison could be detected in them.

The suspicion of poisoning arose in this case from the sudden and violent illness of three persons of the family after a meal, and from its being known that the husband kept a quantity of arsenic and corrosive sublimate in the house for the purpose of destroying the fly in the sheep. On minute examination, however, it was discovered that the mutton of which the family partook on the Monday, was part of the body of a sheep which had been affected with the staggers; and which had in consequence been killed, and the flesh distributed among several poor families in the neighbourhood. It was, however, remarked that none of the other families who had partaken of the food were similarly attacked, nor experienced any ill consequences whatever. It was a portion of the same mutton which had been eaten by this family on the Sunday, and caused the illness of the wife, son, and daughter. The meat had been salted by the wife, and covered over with a tin platter; but as no poisonous substance was detected on the tin plate, nor in the matter vomited or passed by stool, nor in that taken from the stomach and intestines of the boy who died within three hours after partaking of the meal, it appeared reasonable to conclude that the poisonous symptoms were produced by the generation of an animal poison in the meat itself.

Mr A. Taylor comments at some length, and with great ability, on the probable cause of death, and shows that it could neither be attributed to a mineral poison, nor to a natural cause. He then, by a reference to many recorded cases in which poisonous symptoms followed pork, diseased veal, sausages, fungi, &c., points out that the poisonous symptoms were to be attributed to the food itself, either from the animal having been killed when in a diseased state, or from the flesh having become partially decayed.

Particular notice is taken of the cases of poisoning by the use of pork recorded by the late Dr MacDivett, but it does not appear that sufficient attention has been paid to the fact, that in many, if not all, of the recorded cases, where the minute particulars of the state of the pork is described, it has been where the meat has been partially salted. It is therefore an important point for inquiry, how far the mineral salt predisposes fatty substances to undergo changes which shall give rise to the generation of noxious animal poisons. In the only three cases of the production of poisonous symptoms by eating pork which occurred in our own practice, the meat had been rubbed over with salt for two or three days before being used; and during the course of last summer we met with a rather severe case of poisonous symptoms produced by the use of butter which had been kept floating in water in which had been dissolved a handful of salt.

The numerous recorded cases of poisonous symptoms produced by the use of the flesh of diseased animals, seem satisfactorily to establish that the use of such flesh as food is dangerous at all times and ought to be strictly prohibited. In America, however, there are certain regions extending for many miles in length, and some miles in breadth, on the herbage of which, if an animal feeds, its milk and flesh acquire poisonous properties, yet it itself enjoys tolerable health. The disease which the use of the milk or flesh of the animals fed on these districts produces, is known over all America by the name of the Milk-sickness or Trembles. All the infected spots occur west of the Alleghanies, and it is well known that of the early emigrants whole communities, on account of the prevalence of this malady in a particular locality, which is generally distinctly circumscribed, were often compelled to seek another; and, even at this day, those who venture within the boundaries of an infected district are constrained, as a condition of their residence, to abstain from the flesh of the cattle living within the same limits, as well as from the milk and its preparations. It appears from the late Report of Drs Hosack, Post, and Chilton, on this subject, that in some of these infected districts, the inhabitants, with a recklessness of human life which seems incredible, carry the butter and cheese which they themselves dare not eat to the markets of the towns west of the Alleghanies, and that thus there is frequently produced poisonous symptoms and even death, for which the medical attendant cannot account, or is induced to consider as some new or anomalous form of disease. From the same Report we learn that the cattle from these districts are sent in great droves over the mountains; but to deceive the buyers as to the place whence they come, they bring them to New-York by a southern route, and style them "Southern cattle." The flesh of these animals "produce in the persons who make use of it symptoms of aggravated cholera morbus." The viscera of the animals are often found diseased; and the livers almost invariably so.

Seeing these things are so, and that the extent to which poisonous symptoms have been observed to follow the use of such beef, butter, and cheese, has caused the American Government to institute an inquiry into its cause, and that they may probably adopt the recommendation of the learned reporters, it might be prudent in our Government to adopt measures for preventing the introduction of such beef, butter, and cheese into this country; for it can scarcely be doubted that when the enterprising American finds his market shut at New-York, he will salt his beef and send it, with the cheese and butter, to this country.

The next paper by Mr Alfred Taylor is entitled

Cases of Poisoning.—1. *Case of poisoning by Corrosive Sublimate.*—A stout man, 38 years of age, put about two drachms of corrosive sublimate in his mouth, which he masticated and swallowed. He then drank about a pint of water. He was seen soon afterwards, and eggs were administered, after which he vomited, and a mass of corrosive sublimate about the size of a nut was found in the vessel. Large quantities of milk and white of eggs were administered, but the vomiting of yellow-coloured matter mixed with blood continued. Constriction of the œsophagus, with difficulty of swallowing and burning pain, spasms of the lower extremities, hiccough, swelling of the tongue, gums, and lips, with salivation, scarcely perceptible pulse, cold feet, yellowness of skin, delirium and stertorous breathing, ending in death, were the most prominent symptoms. He died four days after taking the poison, and no urine was passed during all that time.

The chief interest of this case lies in the chemical analysis of the solids and fluids of the body, together with that of the contents of the alimentary canal.

No traces of mercury could be detected, on the most minute examination, in the contents of the stomach, nor in the coats of that organ. The blood, the spleen, the *liquor pericardii*, and the serous fluid of the peritoneum, equally failed to exhibit any traces of the mineral poison; in fact, chemical analysis did not succeed in detecting the presence of the poison in any organ or fluid. No mention is made of the kidneys having been tested for mercury, nor yet of the urine found in the bladder,—a circumstance much to be regretted, seeing that, from the circumstance of the urine having been suppressed from the moment of taking the poison, the mercury, if anywhere, would probably have been detected in the kidney, or in its secretion.

The *post mortem* appearances consisted of high injection of the mucous membrane of the stomach with red blood, and marks of inflammation over the whole surface of the stomach. The slate-gray colour, often observed in cases of poisoning by corrosive sublimate, did not here exist. No appearance of corrosion or of ulceration existed. The duodenum and jejunum were healthy; but the lower two-thirds of the ileum were marked by inflamed patches, especially where they joined the cæcum. The large intestines were highly inflamed, and several ulcerations about the size of a pea were scattered over the mucous surface. The liver was enlarged and congested. The spleen natural and firm. The pancreas were healthy. The kidneys were rather florid in colour, and the cortical part presented minute red points. The right kidney had a cyst on its posterior surface containing clear fluid. The bladder was much contracted, contained about half an ounce of turbid urine, and its mucous membrane was slightly injected with blood.

2. *Case of poisoning by Paregoric Elixir.*—The interesting point in this case is the smallness of the dose of opium which proved fatal. A child 5½ years of age had a mixture, containing half its amount of paregoric elixir, (English,) administered in doses of two tea-spoonfuls every four hours. Notwithstanding the production of narcotic effects, the medicine was administered till about five or six doses were taken, and the child died of narcotism within thirty-seven hours from the administration of the first dose. From the quantity left in the bottle, and from its examination, it appeared that death had been produced in this child from the administration of " one grain and a quarter of opium divided in five doses, over a space of about thirty hours, the doses not being given with great regularity."

This is just another of those cases which are continually occurring through the carelessness of apothecaries and practitioners, in which fatal effects follow the administration of doses of opiates above what the experience of the profession has fixed for the ultimatum for that tender period of life. From the known effects of opium on children, had this quantity been given in one dose it would probably have produced a fatal result in six or eight hours. The contents of the stomach had no smell of opium, or of any opiate preparation ; nor did they yield, on a chemical analysis, the slighest traces of morphia or of meconic acid. Had the facts of the case, therefore, not been well ascertained, as well as some of the fatal mixture been left, no certain clue would have been left as to the cause of death, and innocent persons migh have been accused of having made away with the child.

Dr Lever, whose work we had occasion so recently to notice, has several papers in these "Reports." The only one which our space allows us to notice here is entitled

Cases of Puerperal Convulsions, with remarks.—The only point we desire to bring under the notice of our readers, is the fact, for the first time, we believe, noticed by Dr Lever, that albuminous urine is almost always met with in cases of puerperal convulsions, that is to say, that these convulsions occur almost entirely in those labouring under an albuminous state of the urine. Out of 7404 women attended by the pupils attached to the Lying-in-Charity of Guy's Hospital, fourteen cases of puerperal convulsions occurred. In only ten of these cases was the urine examined, and in nine was it found albuminous. These facts, however, by no means prove that the women were labouring under organic disease of the kidney; indeed, the narrative of the cases clearly proves that the albuminous urine in almost all was an accidental and temporary occurrence, probably produced by the very same cause which gave rise to the convulsions. Thus Case 5. had convulsions and albuminous urine on 29th October, but by 3d November the convulsions were gone, and so was all trace of albumen in the urine. Case 6. had convulsions and albuminous urine on 22d November. At noon of the same day, the convulsions being gone, "the traces of albumen in the urine were very faint," and by December 1, all traces of albumen were gone. And so it was of almost all the other cases. That the very same cause gave rise to the production of albuminous urine and to the convulsions is evidenced still farther by Dr Lever's own researches. He states that of fifty women, in whom he examined the state of the urine during labour," in NO cases have I detected albumen, except in those in which there have been convulsions, or in which symptoms have presented themselves, and which are readily recognised as the precursors of puerperal fits."

The discovery of this state of the urine calls for no new mode of treatment. Prompt and decided blood-lettings, cold to the head, antimony, and purgatives, with delivery as soon as circumstances permit, if the convulsions come on before delivery is accomplished, still form the only efficacious plan of treatment.

Dr Lever's other papers are "On Pelvic Tumours obstructing Parturition," and "Cases of Pelvic Inflammation, with abscess occurring after delivery." In the first of these papers he considers the different kinds of tumours of the ovaries, Fallopian tubes, rectum, bladder, of the cellular tissue of the pelvis, and those varieties of pelvic hernia, which may, and do occasionally offer an obstruction to the course of natural parturition. Interesting cases of these different forms are given, and the plans of treatment which were considered most appropriate. The paper forms a valuable addition to our obstetrical knowledge, and is well worthy of an attentive perusal by those who devote themselves to this branch of the profession.

In the latter paper he details the history of cases in which abscesses formed within the pelvis, probably in the cellular substance, and burst variously into the vagina, rectum, or externally through the skin.

Three other papers occur in these "Reports," connected with obstetrical subjects, viz. one by Mr Key, one by Mr Rose, and the third by Dr Oldham. The first of these has already been noticed on a former occasion, viz. Mr Aston Key "on the Extirpation of Ovarian Cysts." The case recorded proved fatal.

Case of Extra-uterine fœtation. By B. C. B. Rose, Esq. In this case the ovum had been arrested in the left Fallopian tube, near its extremity, but the tumour thus formed had burst about the sixth or eighth week of pregnancy, and caused death by the effusion of blood within the peritoneal cavity. The coats of the tube had been distended to the uttermost, and reduced to the most transparent thinness. They had not been strengthened by any deposit of new structure. A clear delicate amnion holding fluid, contained the fœtus scarcely an inch in length. The black pigment of the eyes was discernible; the cord was about half an inch in length, and the extremities were budding in the usual manner. No umbilical vesicle was discovered. The left ovarium was larger than the right, and on cutting through a mammillary projection with a little fringe of membrane coming out from its most prominent part, the characteristics of a *corpus luteum* were apparent. The uterus was twice the size of the virgin one, its walls thick and vascular, and its interior lined with a slightly raised efflorescent membrane.

On Polypus Uteri, and its co-existence with pregnancy. By H. Oldham, M. D. Several interesting cases of this combination are given, but we shall at present simply quote the results at which Dr Oldham has arrived.

"1. That various sized polypi—some very large—may be developed in the uterus during pregnancy, without at all interfering with gestation, or in any way impeding parturition.

"2. That they are to be suspected when the uterus remains larger than usual after parturition, although hard and contracted, and when ineffective uterine efforts continue, attended by hæmorrhage; and that they are liable to be mistaken for secondary fœtuses.

"3. That the distinction between an included polypus, and a second fœtus ought carefully to be made out before any artificial attempts to deliver are begun.

"4. That the hæmorrhage they occasion is sometimes slight, at others sudden and overwhelming; sometimes it occurs immediately after the birth of the child or placenta, at others it is delayed for a fortnight or three weeks; but it is frequently a harassing daily loss, which defies ordinary remedies to arrest it, and usually occurs in gushes.

"5. That the uterus may propel the polypus before the child's head, or, after delivery, it may close over the growth, and remain quiescent, or paroxysms of uterine pain continue with bleeding, or constant unremitting action of the womb, causing severe distress, exhaustion, and death.

"6. That the uterine action may occasion, in some instances, the separation of the growth, in others, the partial or complete inversion of the womb. If the polypi be attached to the cervix, or immediately above it, it may propel it into the vagina, separating perhaps some cellular adhesions, or lastly, that its constant forcing, with the weight of the polypus, may prolapse the uterus, and force the polypus beyond the external parts.

"7. That the treatment must depend on the prominent symptoms, and on the practicability of applying mechanical means; that if no hæmorrhage be present, or it be but slight, every endeavour, by opiates and rest, should be made to quiet the womb, until the shock of parturition, the uterine circulation, and that of the polypus have diminished; that if the hæmorrhage be violent or continuous, and intractable, or if the incessant contractions of the womb threaten the life of the patient, the polypus is to be removed.

"8. That a polypus may be encircled by a ligature, or, after that, be excised, or primarily twisted off, both during, immediately after, or at any time preceding parturition, without the necessary production of bad symptoms, but that the ligature alone, or ligature and excision, are the preferable plans of operating.

"9. That a polypus thus removed is not regenerated, nor does it necessarily cause sterility, or occasion untoward symptoms in succeeding labours." No. 3, p. 134.

The most important paper in the first Number of Guy's Hospital Reports is entitled,

Account of Observations made under the superintendence of Dr Bright, on patients whose Urine was albuminous. By George Hilaro Barlow, M. D.; *with a Chemical Examination of the Blood and Secretions*, by G. O. Rees, M. D. —From the introductory remarks to this paper by Dr Bright, we learn that, to promote the study of this particular class of diseases, permission was obtained to occupy certain wards in Guy's Hospital, from May to October, into which only those were admitted who laboured under albuminous urine. With the concurrence of the other physicians of the hospital all the cases of this class of diseases which occurred were sent to these wards, and while Dr

Barlow assisted Dr Bright, Dr Rees undertook the chemical part of the inquiry. Excepting in the cases of fever and venereal affections, this appears to have been the only attempt made in this country to turn to account the ample resources of an extensive hospital to the investigation of a particular disease; and the success which has attended this attempt amply justifies the wisdom of the undertaking. The paper consists of a minute account of the symptoms which occurred in each case, and of a very few general conclusions which seem deducible from them.

Case 1, was that of a widow who, after severe flooding five months before admission, accompanied with pain in the loins and vomiting, became œdematous, with scanty high-coloured urine, tympanitic distension of abdomen, pitch-coloured stools, cramps in legs and feet, general weakness, &c. When admitted, her face was pale, bloated, and dejected; pitted on pressure; the skin was smooth, soft, moist; the tongue coated in the centre with a rough white fur, and indented by the teeth on the edges; the abdomen was prominent, tympanitic, and with tenderness on pressure in the right hypochondrium. A few days after admission she was bled to relieve pain in the loins and scapula, after which she had an attack of flooding. She had an epileptic fit the next day, from which she recovered, but sunk and died nine days after admission.

Dr Rees detected 0.54 per 1000 of urea in the serum of the blood. The specific gravity of the urine was 1010, and contained 0.30 grains of albumen in two fluid ounces.

Case 2, was that of a boy six years of age, in whom œdema and albuminous urine supervened on scarlatina. Listlessness, drowsiness, and hurried respiration came on, and he died nine days after admission.

Urea was found in the blood, and also in the fluids effused in the ventricles of the brain, pleuræ, pericardium and peritoneum.

The body was emaciated and anasarcous. Considerable effusion of straw-coloured fluid was found beneath the arachnoid membrane of the brain, and in the cavities of the pleura, peritoneum and pericardium. Connected with the *pia mater*, and dipping into the cineritious matter, were several small bodies, some formed of calcareous matters, others of distinct cysts, inclosing a yellowish curdy substance. The heart was enlarged. The cortical substance of the kidneys was pale; in some parts it appeared course, in others more homogeneous than natural. The tubular structure seemed congested, and presented a strong contrast to the pale cortical substance.

Case 3.—A man, 32 years of age, many years ago received a blow on the loins from which he recovered. Eight weeks before admission, after exposure to cold and wet, was seized with cough, dyspnœa, pain in the loins, and weakness in the knees; had frequent nocturnal calls to micturate. The urine was increased in quantity, and was paler than usual. Œdema commenced in feet and ankles, and extended over whole body. Has severe cramps in his legs. His urine was coagulable by heat and by nitric acid. He was bled, purged by elaterium, &c. Some days after, paralysis of the left side of the body came on, for which he was blistered, bled, and had other appropriate treatment; but he gradually sunk, and died about three months after his admission to the hospital.

An apoplectic clot was found in the right *corpus striatum*. There was slight opacity of the arachnoid and some serous effusion. Fluid was effused in large quantities into the pleura, pericardium, and peritoneum. Patches of old pulmonary apoplexy were observed in the lungs, which were fleshy. The left ventricle of the heart was larger, and its walls nearly three times thicker than usual. Atheromatous deposits were remarked on the aorta, which was of greater capacity than the pulmonary artery. The coronary arteries were remarkably large and thick. Deposits of lymph were remarked in the abdominal walls, and on the convolutions of the intestines. The alimentary canal, but especially the stomach, was much narrowed by the contraction of the peritoneal coat, while the great omentum was a mere lamina on the surface of the transverse colon. The kidney, when cut into, was of a dingy yellow, or dirty white colour throughout. All traces of the cortical part seemed to be obliterated, and the tubular structure was very faintly visible. Dr Rees found that each ounce of urine contained 5.2 grains of albumen, but no urea appeared to have been detected in the blood.

CASE 4.—A man, aged 42, had been long subject to diuresis. Had lately been frequently subjected to cold and wet, and since then has suffered from shortness of breath, swelling of ankles, legs and scrotum, and diminished quantity of urine. On admission the urine had a specific gravity of 1021, and was moderately coagulable by heat and nitric acid. Erythematous redness on inner part of both legs came on, which went on to vesications and incipient gangrene. His breathing became more laboured, his pulse small and fuller, and he died about twenty days after admission.

The surface of the body, but especially the right thigh, was marked by dark patches of gangrene. A large quantity of fluid was found in the cavities of the pleura, pericardium and peritoneum, clear in the two first, but turbid and of a dirty brown in the last. Thin filaments of fibrin were effused on the surface of the arachnoid membrane, and that membrane was opake over the sulci of the brain. The brain was softened. The lungs were generally œdematous, and adhered by bands to the pleuræ. The *cordæ vocales* were swollen and injected. The heart was large and its walls thickened throughout; those of the left ventricle were double their usual thickness. Both kidneys were large and lobulated, and finely marked with specks of a pale orange upon a red ground. The cortical structure was hyperæmic throughout, so that the tubular cones were less conspicuous than natural.

CASE 5.—A sailor, aged 56, always made abundance of water. After being wet for thirty successive days, his ankles, feet, wrists and hands swelled, and extended to his arms, legs, and body. His urine was then coagulable. These symptoms were relieved by acetate of ammonia, antimonials and purgatives. After free indulgence in spirituous liquors the general anasarca returned, with bronchitis and emphysema. A *bruit de soufflet* was also audible below the left nipple—synchronous with the pulse. While in hospital he was attacked by bilious cholera, hæmoptysis and cough. After this the urine became less in quantity, acid and pale, and almost gelatinized on exposure to heat, and on the addition of nitric acid. The difficulty of breathing and general anasarca increased, and he died exhausted.

There was considerable watery effusion between the membranes of the brain. The brain was small and pale. A large quantity of clear pale fluid was observed in the pleura and pericardium. A small quantity of turbid milky fluid was found in the peritoneal cavity. Œdema, watery lobular pneumonia, and slight turgescence were observed in many parts of the lungs. The mucous surface of the larynx above the sacs of Galen was tumid from œdema. The heart was much enlarged, the left side being chiefly diseased. The left ventricle was pale and firmly contracted; its lining membrane being cloudy. The tricuspid orifice was wide, and apparently allowed of regurgitation when the ventricle was distended. The surfaces of contact of the aortic valves were perforated. The aorta was large, thick, uneven, opake, and affected with incipient atheroma, and pulmonary artery small in comparison. Each kidney weighed six one-half ounces; their tunics were thin, and raised by cysts of lightish coloured fluid. These cysts were about the size of hazel-nuts, and from twelve to eighteen were on each kidney. The cortical part was of a pale brown colour, dull, and finely granulated. Dr Rees detected urine in the blood; and the urine was found to contain 5.2 grains of albumen in every ounce.

CASE 6.—A washerwoman, aged 45. In consequence of a fall upon the ice was laid up for some weeks, but had never enjoyed the same good health since. Her ankles and legs became weak and swelled, she had frequent lumbar pains, with sickness and vomiting. This was followed by cough and dyspnœa, and increase of the swelling over the whole body, so that the skin of the legs gave way, and allowed a large quantity of fluid to escape. The impulse of heart was natural, but precordial dulness increased in extent. The urine was densely charged with nut-brown lithates. Heat first cleaned it and then threw down a copious cloud; nitric acid rendered it more turbid and deepened the colour. She was subjected to various treatment, and mercury was given so as to affect the gums. From this date she rapidly became worse, and died about a month after admission.

Serous effusion was found beneath the membranes of the brain. This organ was small and soft. Dark-coloured serum was found in the cavities of the pleura, and the lungs were adherent to the ribs, the upper and middle lobe of the left lung being covered with a false membrane. The lungs were œdematous throughout, and the bronchial tubes dilated. Much fluid was found in the pericardium and peritoneum. The heart was larger than usual, but especially the left ventricle. The valves appeared healthy. The peritoneum was scattered over with fibrinous deposits; the intestinal coats partially ecchymosed and œdematous throughout. The kidneys weighed five and a-half ounces each, and the removal of their tunics exposed a coarse granular structure; they were also much injected. Several urinal cysts were observed. The liver was myristicated and beginning to contract.

Case 7.—A sailor, 35 years of age, subject to cough, suffered from dyspnœa, with scanty expectoration tinged with blood; scanty flow of urine depositing lithates, cleared at first by heat, but afterwards becoming turbid; turbidity also increased by nitric acid. The heart's action was irregular, and he soon died from increase of anasarca, dyspnœa, and pleuritic effusion.

There was general œdema, and copious effusion of fluid in the pleura, pericardium and peritoneum. The lungs adhered, and there were traces of old pneumonia in the right lung. At the upper portion of the left lung were several patches of pulmonary apoplexy. The bronchial glands were large and black. White patches of adventitious deposit existed on the surface of the right auricle and ventricle of the heart. The right side of the heart was enlarged, the ventricles large and thick, with the columnæ much developed. The coronary vein admitted the point of the thumb. The left side of the heart was nearly natural. Considerable patches of ossific deposit were observed on the lining membrane of the right auricle. The left auriculo-ventricular orifice was small and contracted; the mitral valve thickened and cartilaginous, with osseous deposits on its auricular surface. The cortical structure of the kidney was coarse and slightly granular; the mucous membrane of the pelvis much congested. The liver was hard, and in an advanced stage of the nutmeg degeneration.

The remarks which follow in these interesting cases are so important, and would suffer so much from curtailment, that we are induced to give them entire:—

"The two cases, in which the complication consisted of affection of the encephalon, illustrate forcibly the danger of the sudden invasion of cerebral symptoms; which always exist where albumen is present in the urine, in any quantity, however small, accompanied with a diminution in the specific gravity of that secretion; a danger which is not diminished by the absence or the subsidence of any dropsical swelling: for, in the former case, it is worthy of notice, that the quantity of albumen in the urine was but small throughout, and that the dropsical effusion had nearly subsided before the more alarming symptoms manifested themselves; and in the second case (that of Henry Stanley), the anasarca was much diminished, and the urine improved in every respect, except its specific gravity, just at the time that the fatal attack occurred. Indeed, I think that, from a careful observation of no very limited number of cases of this disease, I may pretty confidently affirm that in those cases of albuminous urine, in which there is little or no dropsical swelling, with but a moderate or even scanty deposit of albumen, the skin being at the same time moist and perspirable, but the urine defective in its solid contents—as evinced by its light specific gravity, and the absence of urinous odour upon the application of heat or nitric acid—there is more especial danger of the sudden invasion of disease in the brain or its membranes. In one or two very-well marked cases of this kind, almost the only dropsical effusion has been a sort of watery chemosis, produced, as I believe, by anasarca of the cellular membrane under the conjunctiva.

"In Case 3, (Charles Scott), the cerebral symptoms were of a different character from those in the preceding cases, being obviously the result of an apoplectic clot in the very part of the brain in which the situation of the paralysis would have led us to expect it. This extravasation appears to have been the result of disease in the arteries of the brain, as well, perhaps, as of the hypertrophy of the left ventricle of the heart. The origin, however, of this hypertrophy is a matter of some difficulty, as well as of some importance, as the question has been raised; and it is certainly one deserving of grave consideration—Whether, in cases of this kind, where disease of the heart co-exists with disease of the kidney (a coincidence which is observed so often as to render it in the highest degree probable that the two lesions bear some necessary connection with each other) the former or the latter is to be regarded as the primary affection—whether, in fact, the renal disease is any thing more than the result of congestion produced by mechanical obstruction—or whether the disease of the heart can be in any way shown to be the result of that of the kidney? Upon this question, Cases 3, 4, 5, 6, and 7, tend to throw considerable light.

"To begin, then, with Case 3;—and let us, first of all, endeavour to reconcile it with the former hypothesis. The hypertrophy of the left ventricle, then, was notable, and its injecting force must have been excessive: there is, however, much difficulty in accounting for this hypertrophy, as there was no valvular disease; unless we refer it to the morbid changes which had taken place in the arteries and which may certainly be regarded as a real and a sufficient cause for the phenomenon; for it must be obvious that the disease of the arteries was not produced by the violence which might have been done to their lining membranes by the too-powerful ventricles; since, in the arch of the aorta, where such violence would have been the greatest, the injury was less than in the descending portion, where the force of the heart's action must have been far less.

" According to this hypothesis, then, the arterial derangement must have been the primary lesion; which, by the impediment it offered to the free passage of the blood, gave rise to hypertrophy of the left ventricle, which, in its turn, by its too-powerful injecting force, caused a congestion of the kidneys, which led to their ultimate disorganization. It need hardly be remarked, that there is a contradiction involved in the very terms of the explanation: but further than this there is the difficulty of explaining why the kidneys should have suffered so much, whilst the liver, which is generally the first organ to be affected by disease of the heart, should have escaped; and, also, of accounting for disease of the arteries, which there is every reason to believe is a rare affection, when occurring idiopathically.

" Let us now assume, that the disease of the kidney was the primary affection: and in so doing, we may observe that we are not met *in limine* by the same difficulty which we had to encounter on the former hypothesis; for although to assume that the kidneys are often primarily affected with this disease would be, in some measure, begging the question, yet numberless analogies may be drawn from other secreting organs, to show that over-stimulation may give rise to hyperæmia and subsequent disorganization: and that the kidneys were so stimulated in this case, is rendered highly probable, from the history of the invasion of the disease, as well as the previous habits of the patient. It is also ascertained that changes were produced in the condition both of the secretion of the kidneys and of the blood; the former being deficient in some of its natural solid contents, which were found to exist in excess in the blood; whilst the latter fluid was deficient in an important solid ingredient, which was being continually abstracted by the kidneys; so that there was a two-fold change produced in the latter fluid; namely, the presence of an irritating substance, which it is the office of the kidneys in health to remove; and also the absence of a substance, which, independently of other important purposes in the animal economy, gives to the blood a viscidity which (according to the observations of Magendie) renders it more easy of transmission through the capillary vessels. We have, then, a cause sufficient to account for the disease in the arterial tunics, whereby the circulation in the larger vessels must be impeded; as well as a further obstacle to the transmission of the blood, resulting from the change in the blood itself: both of which circumstances must have co-operated in producing the hypertrophy of the left ventricle. It cannot, then, I think, be doubted, that the latter hypothesis accounts for the phenomena in this instance more readily than the former.

"The same line of reasoning, with very little alteration, applies to Cases 4 and 5: though perhaps, in the latter, there might be some suspicion of disease in the chest giving rise to venous congestion in some of the abdominal viscera, and, amongst others, in the kidneys, although not to a sufficient extent to account for the change which those organs had undergone.

"In Case 4, indeed, there had been cough and dyspnœa for some time, before any dropsical swelling manifested itself: but although it is by no means improbable, from the history of the case, that bronchitis existed, yet it is now so well ascertained that renal disease may exist without any dropsical swelling, and also, that, when it does exist, it is so uncertain in its situation, that we are not justified by its absence in assuming that the kidneys were sound, or even that disease in those organs might not have existed to an extent capable of producing œdema of the lungs: whereas the copious diuresis which existed for some time previously would be sufficient to lead us to believe that the kidneys had been for a long time in a state of over-activity—a belief which is most fully confirmed by the advanced state of disorganization in which these organs were found after death.

"In Case 5, again, we have evidence of a still greater amount of obstructive disease in the lungs and heart, though the left side of the latter organ was principally affected; so that, although there existed a cause for venous congestion in the abdominal viscera, and, among the rest, in the kidneys, it was not sufficient to account for the extent of degeneration which these organs had undergone.

"In Case 6, again, we have a still greater amount of obstruction to the return of the venous blood; and, with it, considerable disease of the kidney, which, however, was principally of a congestive character. But it is in Case 7, that we find the causes of venous obstruction in their fullest extent; there being great dilatation and hypertrophy of the right ventricle, owing to the impediment to the pulmonary circulation, caused by the narrowing of the left auriculo-ventricular opening on the right side of the heart: yet here the disease in the kidney was less advanced than in any of the preceding cases, but that disease was of a congestive character, and during life the urine contained a scarcely appreciable quantity of albumen.

"The five last cases, in fact, present a series in which the renal disease becomes less and less, whilst the thoracic disease becomes greater and greater: the former affection, however, assuming more of the congestive character, as we approach the close of the series.

"We are then, I think, justified in concluding, that although disease in the heart or lungs may, by the impediment which it causes to the return of blood through the veins, give rise to congestion in the kidneys in common with other organs in the abdomen, and thereby derange their functions, and in some cases lead to their ultimate disorganization, yet, as such disease was the least in those cases where the renal disease was the most advanced, and the converse, we are not to conclude that such disease in the chest is the universal or even frequent cause of the true Renal Disease; and that we must look elsewhere for the seat of the primary lesion in this formidable malady."—Pp. 222–226.

Next follows the narration of two cases in which purpura occurred as a complication of disease of the kidney. One of these was in a phthisical patient, and in another such case which is recorded, dysentery appears to have carried off the patient. In the two cases in which purpura occurred, that disease is attributed to the "impeded passage of blood through the ascending cava, which must have resulted from the liver having been gorged by long-continued thoracic disease." It is, moreover, remarked as rather astonishing that purpura does not more frequently occur during the course of kidney disease in which the urine is albuminous,—seeing that a tendency to hemorrhage not unfrequently exists in that complaint.

The last case which is recorded under this division is one in which disease of the chest, liver, and kidneys co-existed for a considerable time, but in which it was impossible to decide as to which was the primary lesion. The twenty-four cases whose history follows, are detailed for the purpose of showing the effect of different classes of remedies on the renal disease. They are valuable as enabling us to judge of the comparative efficacy of the different remedies and plans of treatment. The cases are classed according to the leading therapeutical agent employed in the treatment; the first series consisting of

Cases in which Mercury was employed to a sufficient extent to affect the Gums.—The *first* case, a lad of 16, suffering from cough, expectoration, and general œdema and dropsy of the abdomen, had frequent relapses during the treatment, requiring blood-letting, digitalis, squills, and even elaterium. He was, however, at length dismissed from the hospital, improved in health, the œdema having subsided, but with the urine still very coagulable, and with fluid in the abdomen.

The *second* case, a drayman of 31, with severe dyspnœa and cough, and œdema of feet and legs, in addition to the mercury, had laxatives, a blister over the sternum, and ipecacuan with hyoscyamus. The urine ceased to yield albumen, and he made a complete recovery. In his case, the diseased state of the kidney was attributed to congestion produced by the bronchitis.

The *third* case, a coal-whipper of 28, with dyspnœa, cough, crowing respiration, and œdema of feet and legs, in addition to the mercury had hyoscyamus, camphor, ipecacuan, the saline mixture, a blister, and latterly sulphate of zinc with extract of anthemis, and then hydrocyanic acid; but he died two months after admission. His case was considered to be one of congestive disease of the kidney, giving rise to albuminous urine, in a broken-down constitution.

The *fourth* case, a woman of 32, with pain under the sternum, swelling of the feet and loins, and slight giddiness, improved much under antimonials and acetate of ammonia, but did not make any further improvement under the mercurial treatment. The anasarca returned shortly after she left the hospital.

In commenting on these four cases, it is remarked that the principal reason why mercury was not given in a greater number of cases, was the conviction which these trials made on the learned physicians' minds, that "it was at best an inefficient, but in all probability a dangerous remedy, in those cases in which the renal disease was the primary and principal affection; and that even where its exhibition might be indicated by disease in other organs, its effects upon the system should be most carefully and anxiously watched."

Cases in which Elaterium constituted an important part of the treatment.—In this series of cases elaterium was not the only remedy employed, nor even that one which was most efficient in producing amendment, but the results show that it affords a valuable adjuvant means in this formidable disease.

The *first* case, a Dane of 40, subject to inflammatory attacks of the chest, and formerly treated for abdominal dropsy, with dyspnœa, cough, and copious expectoration, œdematous legs, full abdomen, and swollen face, was chiefly treated by elaterium in doses of one-fourth of a grain, three times a day, diminished afterwards to one-eighth of a grain every morning. He had also antimonials and acetate of ammonia. He was considerably relieved by the time he left the hospital; but some œdema of legs and puffiness about the eyes still remained, and his urine was very coagulable.

The *second* case, a shoemaker of 38, with swellings of the legs and ankles, cough, &c. was first put on acetate of ammonia and antimonial wine, under which he improved considerably. He was then purged by elaterium, by which he felt improved. When dismissed, his urine was still coagulable, and the œdema of legs was not quite gone.

The *third* case, a porter of 39, subject to rheumatism, and lately affected with a vesicular eruption, with œdema of the lower limbs, scrotum, and loins, hurried respiration and anæmial bloated look, was purged by elaterium, and had also acetate of ammonia and antimony, and was bled. Occasional doses of compound jalap powder were also given. His symptoms were greatly relieved; and when he left the hospital the œdema was only perceptible in his legs after being up the whole day, and the urine was only slightly coagulable.

The *fourth* case, an iron-founder of 56, with swollen abdomen and legs, and dry skin, was first put on antimonial wine and acetate of ammonia, and then purged by elaterium. Head symptoms threatening he was bled, and had afterwards a blister applied to the nape of the neck. The elaterium, with other purgatives, was continued. He was relieved; but when he left the hospital the urine was still coagulable and the œdema visible.

The *fifth* case, a cooper of 68, with cough and muco-purulent expectoration, and œdema of legs, took an eighth of a grain of elaterium three times a day, and also antimonial wine, acetate of ammonia, and tincture of hyoscyamus. These were afterwards altered for *Pil. conii comp.* as he felt very weak. Head symptoms threatening, a blister was applied to the nape of the neck, when he was also ordered spirits of nitrous ether with hyoscyamus. When the head was easier infusion of gentian with nitric acid was given, under which he gained strength; the cough and œdema disappeared, but the urine still remained slightly coagulable.

The *sixth* case, a hawker of umbrellas, with œdema of legs, ankles, and loins, swelled glands of the throat, pain in loins and head, was bled, and had acetate of ammonia with antimonial wine and elaterium. A twentieth of a grain of this last, increased to a tenth and a fifth, was given. This medicine at length causing nausea, colocynth pills with effervescent mixture were administered, and when nausea abated, infusion of gentian with nitric acid. These remedies produced great relief, and when he left the hospital the œdema had disappeared, and the urine was only very slightly coagulable. The serum of the blood in this case contained 1.25 per 1000 of urea.

Cases in which Antimonials constituted the principal remedy.—From the above cases it will be seen that antimonials were employed as well as the elaterium, so that they might almost have been arranged under this head; more especially seeing that the cases in which antimonials were employed are arranged into classes, according as the antimonials were combined with venesection, with tonics, or with camphor.

1. *Antimonials with venesection.*—Case *first*, a ropemaker of 44, with pain in loins and general œdema, brought on by a fall, slight cough but no expectoration, had acetate of ammonia with antimonial wine three times a day, and compound jalap powder. He was afterwards bled. Under these remedies the œdema disappeared, the urine became natural, and offered no traces of albumen.

Case *second*, a seaman of 45, with œdema of legs, and shortness of breath on exertion, had acetate of ammonia and antimonial wine, with compound jalap powder. The antimony was afterwards given to the extent of one grain with four grains of extract of hyoscyamus, and he was bled. The dyspnœa and œdema diminished, the urine increased in quantity, and was only slightly coagulable by heat and nitric acid when he left the hospital.

Case *third*, a porter of 26, with great œdema of lower extremities, scrotum and penis, troublesome cough with mucous expectoration, old disease of chest, had first some acetate of ammonia, elaterium, and compound powder of jalap. He then had *Hydrarg. cum creta*, with Dover's powder on account of his cough; and in a few days afterwards antimonial wine. Acute pain in the right arm and loins was relieved by blood-letting, and pain in the head by epistaxis, which occurred two or three times. Acetate of lead and opium, with infusion of gentian and nitric acid, were then given, afterwards changed for a preparation of iron with digitalis. Iodine and mercurial ointment were then rubbed over the right hypochondrium, on account of induration of the liver. When he left the hospital the tumour was little diminished; the œdema was, however, much relieved, but the urine was still slightly coagulable.

Case *fourth*, a gas-pipe layer of 32, subject to rheumatism, with great œdema of lower extremities, loins, scrotum, face, tumid abdomen, and pain in the head, was ordered compound powder of jalap and elaterium to regulate his bowels, and acetate of ammonia, antimonial wine, and spirits of nitrous ether. Blood was afterwards drawn to relieve the pain in the head, but when he left the hospital, nearly free from œdema, and with the urine only slightly coagulable, the pain in the head was still complained of.

2. *Antimonials followed by tonics.*—Case *first*, a sailor's wife, aged 27, with dropsy of legs and abdomen, swelling of face, pain and giddiness in head. Had at first diaphoretics and purgatives, and during the time she was in the hospital had an attack of gout. Tonics, what kind is not mentioned, were afterwards given. Her health improved, and the œdema diminished, but when she left the hospital the urine was still albuminous.

Case *second*, a seaman of 32, subject to rheumatism, with œdematous feet and ankles, puffy face, and sense of constriction at *scorbiculus cordis*, was ordered the warm bath three times a week, acetate of ammonia and antimonial wine three times a day. Colocynth and compound galbanum pills, and castor oil were also administered, afterwards elaterium and compound powder of jalap. He improved under this, but fell back again under the use of infusion of gentian with nitric acid. The bitters were therefore stopped, and acetate of ammonia with infusion of diosma substituted. Vertigo and pains in the head required the administration of elaterium; after which he had sulphate of zinc and extract of anthemis. Under this treatment he greatly improved. The œdema left his feet and ankles, but, when he left, his urine was still slightly coagulable by heat and acid.

Case *third*, a maid-servant of 38, with cough, pain in chest, dyspnœa, stoppage of the catamenia, œdema of feet and legs, aching pains and cramps in larger joints, had acetate of ammonia and antimonial wine every four hours, together with camphor and hyoscyamus. This removed the œdema, and she was ordered infusion of calumba, with sesquichloride of iron. She left the hospital improved in health and strength, free from œdema, and with the urine free from albumen.

Case *fourth*, a hair sorter of 61, with general œdema, pleuritic and some abdominal effusion, cough, and dyspnœa, had warm bath, acetate of ammonia and antimonial wine, and occasional purgatives. Under this his health improved, and the œdema became very slight. Infusion of gentian with nitric acid was then given, but not seeming to improve under these, the diaphoretic mixture was again prescribed. Some old ulcers broke out, which caused considerable annoyance. When he left the hospital his general health was improved, the œdema was slight, and the urine less coagulable than on his admission.

Case *fifth*, a milliner of 30, subject to cramps of the legs, and daily vomitings, œdema of legs and eyelids, did not improve while under treatment in the hospital, the coagulability of the urine, œdema, and other symptoms remaining nearly stationary.

3. *Antimonials with Camphor.*—Only one case is narrated under this head, that of a labourer of 52, with hypertrophy of the right side of the heart, dyspnœa, enlargement of the liver, abdominal effusion, and œdema of the legs, was bled and cupped, had the usual antimonial mixture and aperients. Pain in the head, with delirium, and mental hallucinations came on, and continued nearly the whole time he was in the hospital. It is not mentioned when or how the camphor was given. Blisters behind the ears and purgatives seemed to afford some relief to the head symptoms. When he left the hospital there was no appreciable œdema, the urine was only rendered opalescent by heat or nitric acid, and he only occasionally wandered during the night.

Cases in which Tonics were the remedies principally employed.—Case *first*, a woman of 37, with syphilitic nodes, &c. of old standing, pains in the loins, frequent vomitings, pains and startings in the limbs, legs œdematous, irregular action of heart, had Dover's powders night and morning, the antimonial mixture and the tepid bath. Pain and tenderness over the parietal bones were relieved by iodide of potash. The œdema was at first relieved, but afterwards returned, attended by effusion of fluid within the abdomen. It is not stated what tonics were given, nor when. She left the hospital considerably relieved, but the urine remained nearly as coagulable as on admission.

Case *second,* a shipwright of 40, with swelling of the abdomen and lower extremities, palpitations, heart's action diffused over the chest, and attended by unnatural sounds, was first ordered sulphate of iron and extract of gentian, with compound infusion of gentian.

He afterwards took citrate of iron, with infusion and tincture of calumba and laxatives. Camphor and hyoscyamus were afterwards given, changed for sulphate of zinc, and then again given. The urine became more coagulable, and the œdema was still visible on legs and loins. Spots of purpura were then discovered on the legs, on which he was ordered decoction of cinchona, *spiritus armoraciæ comp.*, and sesquicarbonate of ammonia thrice daily. A blister to the chest and other remedies were tried, but all with little benefit. Each ounce of his urine contained 0.65 grains of albumen.

Case *third*, a woman of 24, suckling her second child, with œdema of legs and loins, but no tenderness in the loins, with excoriations on the legs, from which fluid escaped, irregular action of heart, and great debility, was put immediately on tonic and stimulating diet. She had aromatic confection, with ammonia in infusion of cusparia, besides wine, porter, and meat. She steadily improved under this plan of treatment, the sore on the leg healed, and she left the hospital in apparently good health. When first admitted urea was detected by Dr Rees in the milk. The urine of the child which she nursed was not albuminous.

Case *fourth*, a brushmaker of 49, with hæmoptysis, epistaxis, cough, pain at pit of stomach, dyspnœa, œdema, impetiginous eruption on legs and feet, and enlarged liver, was bled, purged, and had acetate of ammonia with spirits of nitrous ether and camphor mixture; afterwards the compound infusion of roses, with sulphate of magnesia. The hæmoptysis still continuing, he had dilute sulphuric acid, with infusion of orange peel, and *Spir. armoraciæ comp.* Under this treatment the œdema and hæmoptysis left him, and at the time he left the hospital no albumen could be detected in the urine. We are informed that this man died of renal disease a few months afterwards.

Such is a very short abstract of these interesting cases. In the whole the urine was albuminous, though to save our space we omitted to notice the fact in the abstract of the cases. Much has of late been talked, and more written, relative to the low specific gravity of the urine in this disease, but as facts are the things wanted, and not arguments, on a matter of this kind, we give an abstract of the ascertained specific gravities of the urine in the thirty-seven cases narrated in the table at the end of this paper. In 1 case only, the specific gravity was so low as 1004. In 3 cases it was 1008; in 3 cases, 1009; in 4 cases, 1010; in 5 cases, 1011; in 1 case, 1012; in 5 cases, 1013; in one of these, however, varying to 1026; in 2 cases, 1014; in 1 case, 1015; in 5 cases, 1016; in one case, 1017; in 1 case varied from 1019 to 1026; in 1 case, 1021; in 3 cases, 1025; in 1 case, 1027; in one case, 1030; in 1 case, 1032; in 1 case, 1034. From this simple statement it is apparent that no conclusion could be arrived at from the specific gravity of the urine alone, seeing that the full half of the cases gave a specific gravity equal to the average of healthy urine. Besides it is a most remarkable fact that the specific gravity of that fluid varied most materially on different days, appearing in general to follow the very same law which the healthy secretion does, viz. diminution of specific gravity with increase of the amount passed, and *vice versa*. As might be expected, however, in a disease of this kind, the above law underwent many exceptions. Thus, in one case, when forty-two ounces were passed in 24 hours, the specific gravity was so great as 1021, whereas, when the quantity afterwards became reduced to 28 ounces, the specific gravity fell to 1009.

This paper is illustrated by several beautiful engravings of diseased kidney, and is followed by

Observations on the Blood, with reference to its peculiar condition in the Morbus Brightii. By George Owen Rees, M. D., &c. After some preliminary remarks as to the constitution of the blood corpuscle, Dr Rees endeavours to show that it consists of a sac filled with colouring matter (the hæmatosin), the sac forming the white matter of the corpuscles. To prove this point a clot of blood was washed in serum, and the red particles allowed to settle to the bottom. The serum was then decanted, and the red deposit poured into distilled water, and well agitated. A white deposit fell to the bottom of the vessel, while the supernatant fluid remained clear and of a deep-red colour. The deposit was found to consist of the white sacs of the blood corpuscles. Some experiments are then detailed to show that the membranous sac of the blood particle allows exosmose through it the same as other animal membranes. By steeping the blood particles in saline solutions of various strengths, conditions were manifested in the blood particles, and in the solution, which showed that a certain amount of interchange of the fluids had taken place.

Dr Rees then shows that when a chemical examination is made of the red fluid extracted by means of distilled water from the blood particles, it is found to contain the whole iron of the blood, while not a trace is furnished by the white deposit, or sacs of the blood particles. Dr Rees thence infers that the smaller blood corpuscles must attain maturity by iron entering within the envelop to supply one of the constituents of the colouring matter. To insure this, a liquor containing iron in solution must be applied to the membrane of the corpuscles, and this liquor must be of a specific gravity less than that contained within these bodies. A very forced explanation is had recourse to, to explain how these objects may be attained. He believes the iron enters the blood with the chyle and lymph, which enters the blood through the thoracic duct, " to which fluid all experimenters have given a lower specific gravity than the *liquor sanguinis*." The iron, in fact, he supposes to be disposed in this fluid in the shape of a lactate. "Nature has, then," says he, "in this admirable manner, provided for the introduction of iron into the corpuscle, by presenting it in a perfectly soluble form to the enveloping membrane, and dissolved in a liquor of a specific gravity suited to effect the necessary accommodations." The fallacy of such a supposition must be apparent to every mind; for where could the blood particles circulate in order to get free of the serum in which they float, and become surrounded with the lacteal fluid? Or, in the ordinary mode in which the lacteal fluid and venous blood mixes, how could that fluid so perfectly surround the particles as to allow the above supposed endosmosis to occur? Then, again, in those diseases in which there is a manifest deficiency of iron in the blood, how would the above assumptions explain the reintroduction of iron into the blood particles? It never could; for if any one fact is better established than another it is this, that the medicinal agents, iron as well as the rest, are removed from the stomach directly by the veins, and never in any appreciable quantity pass through the lacteals, or become an integral portion of the chyle. If, therefore, the blood particles receive an additional supply of iron, they must receive it, not through means of the chyle, but through the agency of the serous portion of the blood; and if we allow the blood particles to be endowed with the principle of vitality, which all must admit, we must also allow them to be endowed with the principle of election, by which they are capable of selecting from the serum those principles requisite for their perfect development.

Assuming, however, his position as proved, Dr Rees proceeds to apply his theory to the condition of the blood in disease, more especially in the *Morbus Brightii.* "Let the blood become deranged," says he, "so that its specific gravity is lessened, and we may feel assured, that if the physical qualities, more especially the specific gravity of the chyle, be not simultaneously affected, and that too in a due proportion, the result must be, that the red colouring matter, the great oxygenator of the blood, is no longer produced in its ordinary quantity; the ferruginous serum of the blood not being able to enter the blood-corpuscle as in health. Again, if the degeneration above alluded to take place, we must recollect that all the solids of the body through which the blood courses are formed with pores and of material admitting of endosmodic action; and that it is impossible for the solid constituents to preserve their health if constantly acted upon by the blood at a specific gravity of 1030 to 36, instead of 1052 to 57; the equilibrium of health being no longer preserved, and the watery blood inducing a like condition in the other solids."—P. 325.

Dr Rees then endeavours to show, by a reference to his experiments on the blood and secretions in that disease, that the following conclusions may be drawn.

1. That there is an excessive quantity of water in the blood.
2. That urea exists in the blood.
3. That urea is also found in the milk and in the fluids effused into the various serous cavities.
4. That one or more of the natural constituents of the urine are absent or deficient in quantity.
5. That the urine is more watery than natural.
6. That albumen is found in the urine.

Dr Rees' experiments are, however, by far too few in number (only six apparently,) and not sufficiently extended over the stages of the disease in the same patient, to induce us to adopt his conclusions in preference to those of former observers. The carefully conducted experiments of Dr Christison, to instance no other, proved that the chemical constitution of the blood is far from being the same in the different stages of the disease. Thus, the solid albumen of the serum, which, in the early stages, is invariably reduced below the natural standard, "gradually returns to the healthy standard, or even exceeds it," as the disease progresses. Nay more, it even appears that in the most advanced stage, where there was no reaction and very little coagulability of the urine, "the density of the serum was 1030, and the proportion of its salts and albumen to the entire blood so high as 973 in ten thousand parts," a quantity considerably above what Dr Christison ascertained to be the healthy standard, which varied from 816 to 853. The same variations attended the relative proportions of the other ingredients of the blood in the experiments of that accurate inquirer, but the above single fact is mentioned to show the fallacy of any one endeavouring to create a theory before he has a sufficient quantity of facts to show that that theory is capable of explaining the general ascertained phenomena of the disease.

There is only one other circumstance which it is necessary to notice in Dr Rees' analysis of the blood of the patients with Bright's disease. In all his experiments he appears to have neglected to separate the fibrine from the blood globules, and this greatly deduces from their value. This circumstance has even led him into error, as in one of the cases in which he found 144 parts of "fibrin and corpuscles" in 1000 parts of blood, he attributes the great amount of solid matters to the presence of the fibrin, whereas even the largest amount of that substance ever yet found in inflammatory blood would only account for eight parts of the 144. If conclusions worth anything are therefore to be drawn from chemical experiments on the constitution of the blood in disease, they must have reference to every separate ingredient of that compound fluid. The processes followed ought also to be carefully described, as not only a different result is sometimes yielded by following different modes of analysis, but it would enable others to authenticate the facts by repetition of the experiments in other cases.

Observations on Pneumonia and its consequences. By THOMAS ADDISON, M.D.—The simple pneumonia of Laennec is shown by Dr Addison not to be a simple form of that complaint, but a complication—a broncho-pneumonia; and that a truly simple pneumonia is not unfrequently met with in young persons, and in good constitutions, unattended by either pain, cough, or expectoration, or, at least, such a degree of either as to attract particular attention.

Dr Addison then shows that there is no such structure in the lungs as that to which the terms parenchyma or interstitial have been applied. Accompanied by a corresponding branch of the pulmonary artery, a filiform bronchial tube may be traced to a lobule or bunch of cells in which it abruptly terminates; the blood distributed over these cells being received by the pulmonary veins which pass exteriorly to the air cells in a loose and very distinct interlobular cellular tissue. It is in these air cells that true pneumonia is situated, and into them that the ordinary pneumonic deposits are poured. Dr Addison has not, however, succeeded in arriving at any very positive conclusions as to the elementary tissues which compose the air cells, or the exact construction and arrangement of the cells themselves. "In the meantime," says he, "without venturing to decide whether the innumerable, minute, irregular, and manifestly elastic air cells, constituting an individual lobule, partake more of the character of areolar tissue or of a serous membrane, I am fully persuaded that, *pathologically*, they present none of the attributes of a mucous membrane, as Reissessen and others would lead us to believe; and that, so far as the changes induced by inflammation and other diseased conditions are entitled to guide us, they must be ranked with one or other of the first-named tissues. Neither do these changes enable us to recognise any of that intercellular tissue which has been confidently asserted to be the seat of some of the most important diseases of the lungs, and into which, accordingly, the morbid deposits resulting from inflammation and dropsy have been supposed to be effused. So far as my observation has extended, I should have as little hope of finding this interstitial material in a pulmonary lobule as in a mere bundle of areolar or cellular tissue." No. 2, p. 367.

The effects of pneumonia are next described. Dr Addison corroborates Dr Stokes' opinion that an excited state of the respiratory act, and a preternatural dryness of the air cells, as indicated by a murmur louder than natural, is observed in the earliest stage of pneumonia. This fact, however, does not bear on the nature of the tissue of the air cells. Immediately succeeding this, symptoms of engorgement take place, and the examination of the lungs at this stage proves the air-cells to belong to the serous membranes. This effect consists of effusion of serum into the air-cells themselves, and on Dr Addison collecting some of the fluid and heating it, he found it to be highly coagulable. That this fluid is in reality poured into the air-cells themselves may be satisfactorily proved by forcing, by gentle pressure, the effused serum from the cells through the truncated bronchial tubes of the incised lung, and subjecting it to examination. It is often stated that if this were the case it would be expectorated at this stage, whereas expectoration is generally absent. Dr Addison answers to this, that expectoration can only take place when a sufficient quantity of air gets behind the effusion to force it onwards; besides, that in the worst forms of œdema of the lungs, when the cells are full of fluid, there is often little cough, and no expectoration, from the same cause, or if expectoration be present, it is not necessarily of serous fluid. To prove the seat of œdema, Dr Addison, with the aid of Mr Hilton, has both collected the fluid from the truncated bronchii, and found it to consist of serum, and also carefully separated a distinct lobule, and by puncturing it and collecting the fluid, found it also to be serum. The effusion was never found in what has been termed the interstitial tissue.

In the second stage of pneumonia—that of effusion—the cells are injected, red, more substantial or dense to the feel, though still slightly crepitating. Soon, however, the turgid parietes of the cells obey the same laws as other inflamed parts; they lose their natural cohesion, swell, and encroach on the cavities of the cells; cause the absorption of the fluid previously effused, and by thus filling up the cavities, occasion first a dryness and brittleness, and afterwards a complete consolidation, which constitutes the red hepatization of authors. As might be expected, this is attended by such a loss of cohesion as causes the lung to break down readily under pressure. The inflamed part under the microscope presents a granular appearance, but the consolidation does not depend on the actual effusion of solid albuminous matter into the cells.

In proportion as the disorganizing process continues the parietes of the cells become more and more opaque,—how far from a merely molecular change, or how far from superadded albuminous deposit, it is difficult to determine; the blood-vessels become invisible; the parietes of the cells thickened and softened; and with this extreme loss of cohesion, and of vascular turgescence, we have undeniable proofs of albuminous matter being poured into the cavities of the cells. This constitutes the gray hepatization of authors.

When the inflammation occurs in good constitutions, albuminous matter, more or less solid, may be pressed out, and careful washing will sometimes imperfectly restore the cellular appearance of the lung. In bad constitutions, however, the effusion is more fluid, or resembles a dirty-yellow purulent matter, and may readily be shown to be contained in the cells themselves by forcing it by gentle pressure from the cells, when it will escape through a cut tube, or by pricking the cells. These changes may take place over a considerable portion of the lung, rendering the whole of an opaque, white, or gray colour, obliterating, or rather concealing the lines of common cellular membrane which separate lobule from lobule. In other cases, however, the process is confined to individual lobules more or less remote from each other, the inflammatory process being stopped and confined by the common cellular membrane.

The formation of an abscess in good constitutions is extremely rare, but in bad or broken-down constitutions the inflamed parts may terminate in the formation of abscesses, or even of diffuse gangrene. Circumscribed gangrene Dr Addison suspects is always preceded by pulmonary apoplexy. The effused blood excites inflammatory action of the neighbouring parts, acting as a foreign body, becomes putrid from its communication with a bronchial tube, giving rise to all the symptoms of gangrene of the lung. Highly beautiful coloured plates of these different appearances are given at the end of the paper.

Lobular pneumonia, or pneumonia limited to separate lobules more or less remote from each other, is chiefly observed in bad and cachectic habits of body, and especially towards the termination of various chronic diseases, after surgical operations, and in phlebitis. In such cases the vital properties of the tissue are rapidly exhausted, and remarkable softening or small sloughy abscesses very speedily result. That these abscesses do not result from matter being formed in the vein and being mechanically deposited in the lungs, is proved by the circumstance occasionally observed, that in different lobules the progressive changes which precede the formation of the abscess may be traced. A beautifully coloured plate exhibiting this is appended to the paper.

Carnification of the lung was regarded by Laennec as the result of pneumonic inflammation, modified by pleuritic effusion. Externally the lung has a bluish tint, not unlike that of the spleen; it feels soft and flabby, and is remarkably tough. It does not crepitate, is destitute of air, and sinks in water. Internally, when cut into, it presents an uniform texture of a dark or brownish hue. In this case it is usually considered that the pneumonic deposit has taken place in the interstitial tissue. Dr Addison, on the other hand, gives a very different account of the causes of this peculiar appearance. He never could understand how an inflamed tissue could retain its normal tenacity, much less how inflammation should produce an increased tenacity, as the lung has in this case; for loss of cohesion is the invariable result of inflammation in every tissue. He, therefore, accounted for the carnification on principles of simple compression. In cases where this state occurs, the pleuritic effusion has compressed the lung, forced out the air, and brought the sides of the air-cells into close contact. He accordingly took a lung so altered, which instantly sunk in water, and, by the aid of Mr Hilton, inflated it. It inflated perfectly, and the aerial cellular tissue was found in its normal condition, with the exception of slight redness indicative of an unusual quantity of blood.

The peculiar congested state of the most depending portion of the lungs, as seen after death in some cases, is alluded to for the purpose of showing that in some cases it is mixed up with a certain degree of actual inflammation of the air-cells, as indicated by a marked difference of colour, by the dryness and brittleness of the tissue, and by the occasional occurrence of an albuminous deposit, or purulent infiltration in certain points. The difficulty is to determine in such cases whether the congestion gave rise to the pneumonia, or whether the congestion was purely cadaveric.

Such are the most important of Dr Addison's views relative to the seat and nature of pneumonia. The remainder of the paper is taken up with descriptions of the various permanent effects produced in a lung by pneumonia. Dr Addison thinks the nature of these permanent effects depends on the state of the patient's constitution and the character of the inflammation, and consequently upon the nature of the albuminous deposits, and the degree of organization of which that albuminous deposit is susceptible. Of these permanent effects he distinguishes three varieties; 1. The uniform albuminous induration; 2. The granular induration; 3. The gray induration.

When acute pneumonia has supervened in a healthy constitution, the softened tissues become so blended with, or assimilated to, the permanent albuminous deposit, that the whole is converted ultimately with an uniform, homogeneous, semitransparent, or opaque yellowish material, in which not the slightest traces exist of the air-cells or of the interlobular cellular tissue. Vessels carrying red blood may occasionally be traced through this structure. This change may be limited to a few lobules, may involve several, or may be diffused over the greater portion of the lung. This is what Dr Addison terms the uniform albuminous induration.

When, again, the inflammation pours out a less organizable albumen, as often happens in strumous habits, the interlobular cellular tissue generally remains distinct, and a solid, pale, or yellowish, friable, albuminous matter occupies the lobules, and this apparently without having assimilated the parietes of the cells as in the former case. The cellular arrangement is still perceptible, both on the exterior and in the interior of the lobule, giving to the lobule something of the configuration of a raspberry; and when the lobule is cut through, the friable albuminous matter presents a granular appearance, produced by the filling up of the still separate cells. So little is the interlobular areolar tissue affected by this change in the air-cells, that, after keeping the lung for a few days to soften it, these lobules may be fairly turned out and examined. A beautiful plate of this form is given, in which the lobules so turned out are represented of their natural dimensions, and also magnified. It is to this form that Dr Addison gives the name of granular induration; it is sometimes called by others inflammatory tubercle.

The gray induration, again, is made up of a mixture of yellowish-white and black matters, in variable proportions, the tint depending on the greater or less proportion of each. The lighter varieties pass into the uniform albuminous induration, the darker varieties pass on to iron-gray or black, and are sometimes as hard as cartilage. The disorganization of the tissues is less complete than in the uniform albuminous induration, but the albuminous deposit is more organizable than in the granular induration. Dr Addison, in fact, states that in this case it may be said that the pneumonia has terminated in adhesion.

A few valuable remarks follow on the resemblance, almost amounting to identity, observed to exist between the effects of tubercular disease occurring in serous membranes, and those met with in the lungs; and the learned writer shows that the several forms, changes, and varieties presented by the one are almost equally observable in the other.

The morbid structures above described maintain their integrity by a very slender power; the vital agency which holds their molecules in a solid state is liable to be impaired by the very slightest disturbing causes. The most common of these are inflammation of the surrounding parts and a cachectic state of the body; but more especially a concurrence of these two. Under such circumstances, these structures and deposits soften down, and are gradually converted into an albuminous or puriform fluid, and thus give rise to symptoms of phthisis. According to the nature of the induration, and the more or less complete discharge of the softened matter by the bronchial tubes, we may find, on dissection, either one large, round, oval cavity, generally lined or bounded by an opaque moderately thick membrane, the rest of the lungs being healthy; or several adjacent cavities communicating with one another by bronchial tubes.

That form of phthisis which results from the softening of the granular deposit is much more common than the preceding; and Dr Addison thinks "is generally fatal, unless, indeed, it be very circumscribed, as, for example, in one of the apices of the lung;" whereas, of the first form above-mentioned, "not a few completely recover to die of some other disorder."

Relative to the cicatrization of a vomica in the lung, Dr Addison differs from Laennec and Dr Stokes. He believes that such a thing never takes place, and that the appearance which has been mistaken by these pathologists for a cicatrix is nothing else than the uniform albuminous or gray induration distributed, as it frequently is, in a linear form, so as very much to resemble a cicatrix. "The perfect identity of the physical signs of pneumonic consolidation with mucus in the tubes of the consolidated part, and those of pneumonic abscess on the one hand, and the linear distribution of pneumonic deposit, with consequent puckering of the lung on the other," have led him to the belief that "many at least, if not all the cases of cicatrized abscess may have been of this kind."

Observations on Lithotomy. By BRANSBY B. COOPER, F. R. S.—Mr Cooper's celebrity as a practical surgeon gives an extreme interest to his observations on lithotomy,—an operation which has been the subject of a practical treatise by almost every celebrated operator since the days of Cheselden. We do not purpose, however, to give an abstract of his paper at present, seeing his line of incision and mode of operating does not differ from that usually followed; and that this paper is commented on in our last Number by Dr Keith, in his valuable paper "on the Lateral Operation of Lithotomy." Two points alone would we desire to notice, on which Mr Cooper and Mr Keith agree; these are the urgency with which both insist on a careful previous preparation for the operation, and the speedy return to a nutritious diet as early as may be after the operation is over. On both these points, however, Dr Keith is more particular than Mr Cooper, and his cases, especially those which occurred in old persons and in broken down constitutions, showed the very great advantage of previous preparation.

At the time Dr Keith published the first part of his paper (June 1844), the most successful practice of the British surgeons gave a mortality of 1 in 7⅞ cases operated on, while over France the average was 1 in 4½. His own success by his new line of incision and particular mode of opening the prostate gland, was, in hospital practice, (the only one he has published), only 1 in 10½.

Mr B. Cooper says he has "performed the operation eighty-four times at Guy's Hospital, and upwards of twenty in private, with the loss of ten patients." This would give a mortality for the operation of 1 in 10.4, almost the exact mortality in Dr Keith's cases. It were therefore very much to be desired that, instead of the very short note above quoted, Mr Cooper had given a list similar to that of Dr Keith, seeing that of the seven fatal cases which he does record, it most strangely happens that all occur either in the years 1827, 1828, and 1829, or 1837 and 1839, unless this last be a misprint for 1827 and 1829. We are induced to make this remark from the circumstance of the ratio of mortality among his cases appearing so very low as compared with surgeons of fully greater note in the capital; seeing, too, that his line of incision and mode of opening the prostate is identical with theirs. Besides, it does appear extraordinary that the surgeon of a large hospital should have passed a period of seven years, from 1829 to 1837, and then from 1839 to 1844, without meeting with a single fatal case in all that period. We shall be delighted to hear the further particulars of Mr Cooper's success, because it will enable us to judge more fairly between the merits of the old line of incision and present mode of opening the prostate, and that recommended and practised so successfully by Dr Keith.

Report of Cases of Stricture of the Urethra, Retention, and Extravasation, treated in Guy's Hospital from Oct. 1841 *to* 1843. —We are induced to notice these cases for the purpose of suggesting that in such numerous reports of cases, some general statements should accompany them, classifying the cases according to their severity, their cause, or the kind of treatment requisite for their relief. A simple history of eighty-eight cases without one comment on these heads, or on the mortality attending particular classes, is of little practical utility; and few men in practice will be met with who would wade through a tedious repetition of symptoms in eighty-eight cases, in order to pick up any useful hint which might be derived from them. Accompanied with such comments, however, the histories of the individual cases would prove invaluable, as exhibiting the particular modifications which the general treatment of each class required, and the amount of success attending each plan of treatment. Did our space permit we would enter at some length into this subject, as it rarely happens that such an amount of really instructive cases are brought individually before the notice of the profession.

The above title gives a false idea of the cases treated. The great proportion are simply cases of stricture, in which neither retension nor extravasation occurred; the retention, at least, was no more than what every case of stricture produces. Others were cases of stricture complicated with abscess or fistula in the perinæum; and a third and very small class, were cases of stricture complicated with retention and extravasation of urine into the cellular tissue.

In order to give a general idea of these interesting cases, we have been at considerable pains to classify them in the way which ought to have been done by the reporters or editors. There are in all 88 cases; of that number 64 were cases of simple stricture; 16 were cases of stricture complicated with the formation of an abscess, or with fistula in the perinæum; 5 were cases with retention and extravasation of urine into the cellular tissue of the perinæum, scrotum, &c.; 1 was a case of retention produced by the presence of an abscess formed in the perinæum; 1 was a case of retention from injury of the urethra; and 2 were cases of retention from enlarged prostate.

If classed according to the principal means used for the treatment, we find that in 58 cases the catheter or bougie proved the chief means of cure; in 7 cases leeching the perinæum, the hot bath, opiates, and fomentations constituted the main treatment; in 4 cases caustic was applied to the stricture; in 15 cases the abscess of the perinæum, or urethra from the perinæum, was cut into; in 1 case the stricture of the urethra was forced by a conical wound; and in 1 case the stricture was dilated with the knife.

The case of retention from the formation of a perineal abscess was relieved naturally by the bursting of the abscess.

The results of the various kinds of treatments were, that 19 were cured; 54 were relieved; 3 were unrelieved; and 12 died. The mortality of these cases thus amounted to 13.6 per cent.

Inquiry into certain of the Causes of Death after Injuries and Surgical Operations in London Hospitals, with a view to their prevention. By NORMAN CHEVERS, M. D.—This is an important paper, and one filled with pathological and hitherto almost unnoticed facts. It is one which throws quite a new light on the fatality attending surgical operations, and bids fair, by attracting the attention of surgeons to the facts so ably brought out, to prove of no small practical importance. As our space prevents us from noticing the valuable preliminary observations, we must confine ourselves strictly to a few of the more important facts.

Dr Chevers remarks that a very great proportion of those who die from the secondary effects of mechanical injuries have been the subject of some marked, and often very acute form of renal, hepatic or splenic disease, or all of these combined. To illustrate this fact, Dr Chevers has brought forward the history of all the fatal cases in which an inspection was allowed, which occurred in Guy's Hospital for a period of 15 years. These cases amounted to 153. Many of them had undergone severe operations, or suffered from extensive accidental injuries; others had been the subjects of wounds or contusions of a trifling nature; still, the internal inflammations which destroyed life in most of the latter cases, were generally as severe as those which proved fatal in the former, and often more so.

Of the 153 cases, it was found that 134 died from inflammation of secreting surfaces or of internal organs; and the remaining 19 died from various causes, as gangrene, hæmorrhage, tetanus, erysipelas, &c. In very few cases was the inflammatory affection found confined to one surface or organ; several important parts were almost uniformly observed to be equally affected, and these often at a distance from each other and from the seat of the injury.

The following table gives a list of the number of times the various recent inflammatory affections were observed in the 134 cases.

Acute disease of the lungs, appearing in the form of inflammatory œdema, red or grey hepatization, abscess or gangrene, was noticed in	47 cases
Bronchitis,	2
Pleuritis,	35
Laryngitis and Diphtheritis	2
Meningitis,	27
Inflammation, softening, or abscess of the brain,	9
Pericarditis,	14
Peritonitis,	52
Arteritis and Aortitis,	4
Phlebitis,	3
Inflammation of various portions of intestinal canal, excluding cases of Hernia,	9
Suppuration in the substance of the Psoæ muscles	2
Acute purulent Synovitis,	1
Inflammation of the Tunica Vaginalis,	1
Cystitis,	8

In the above enumeration no mention is made of the state of the liver, kidneys, and spleen; they are reserved for a separate table, which includes the whole 153 cases.

The kidneys were observed in a state of marked disease, either congested, softened, mottled, or with the granular or cystiform degeneration in	72 cases
Kidneys healthy in	26
Condition of kidney doubtful in	11
Condition of kidney not ascertained from autopsy being partial in	44

Of the above cases in which the kidneys were either not examined, were healthy, or in a doubtful condition, there was found marked disease of the liver or spleen, or both, in 21 cases, giving a total of 93 cases in which one or more of these important organs were found in a diseased state.

It further appeared, that of the 184 cases which are above enumerated as having died from internal inflammation, in no fewer than 90 there were superadded marked disease of the kidneys, liver or spleen, or of all combined; and there cannot exist a doubt, but that had these organs been examined in all the cases, several more would have been recorded as labouring under diseased kidney.

It is remarked, that in a large proportion of these cases the disease of the liver, spleen, and kidneys, had evidently existed for a very considerable time previous to the patients receiving the wounds or injuries which became the apparent primary causes of death. In very many, however, and this especially in the renal cases, the changes were evidently of so recent a nature as to render it probable that almost immediately after the operation or accident, the visceral disease had been excited from a latent to an active condition; or that acute congestion had then suddenly supervened.

Dr Chevers states, that an attentive study of the pathological appearances after death from surgical operation, has long since convinced him not only that the great proportion of the fatal cases results from existing disease of the kidneys, liver and spleen, called into activity by the operation or injury, but also that every wound, however trifling, is extremely liable to become fatal in those persons whose kidneys or other glands are in a state of congestion. His data for these conclusions are much more satisfactory and complete, with reference to the disease of the kidney as than regards the morbid states of the liver or spleen; and the similarity of the morbid appearances presented by the serous surfaces in those who are cut off by Bright's disease, and those who die from inflammatory attacks after surgical operations or injuries, is justly held to be a strong corroborative proof of the correctness of his conclusions. Various interesting facts are stated in support of these views; and the paper is terminated by suggesting as the best mode of preventing such fatality, that before operating on any patient, means should be used to rectify, so far as possible, any tendency to disease of the viscera to which, on examination, the patient may be found predisposed.

Such are some of the important papers published in the three numbers of the new Series of Guy's Hospital Reports. Our want of space compels us to defer at present any notice of the remainder till some more convenient opportunity.

Art. II.—*Report on the Sanatory Condition of the Labouring Population of Great Britain. A Supplementary Report on the results of a special Inquiry into the Practice of Interment in Towns. Made at the request of Her Majesty's Principal Secretary of State for the Home Department.* By Edwin Chadwick, Esq., Barrister at Law. Presented to both Houses of Parliament by command of Her Majesty. London, 1843. 8vo, Pp. 280.

If in this country it be difficult and expensive to live, it is certainly not less difficult and expensive to die and be buried.

The difficulty and the expense of living comfortably among the great body of society,—the working classes and those a little above them,—has been so long a subject of complaint, and is so generally admitted, that it is idle to attempt to prove or illustrate the fact. There is, in the *first* place, great difficulty in obtaining employment sufficient to maintain steadily and certainly, without fear of interruption, individuals; and it is infinitely more so when these individuals have to maintain families. This is true, not only of the working classes, but of those a little above them,—of the immense number of individuals who are endeavouring, by dint of the possession of a little education, to obtain employments in those departments of trade, commerce, or manufactures, where a certain amount of physical labour is combined with a considerable degree of mental labour continued through many hours of the twenty-four.

Secondly, when occupation sufficient to provide food, raiment, the shelter of a roof, and all the other contingencies implied, is obtained, such are the habits of society even in these lower regions of the Pyramid, that much expense is incurred, not for mere necessaries and comforts, but for the supply of various artificial wants, which, whether justly or unjustly, are considered to be necessary in the present constitution of society. Of the slender earnings of the young man behind the counter, in the counting-house or the factory wareroom, a proportion is employed not only in providing raiment of a certain quality, but part goes to purchase other adventitious articles;—as ornaments; a certain proportion, of cigars in a smart cigar case, and the means of an occasional draught of half-and-half. Public amusements also come in for their share; and, in short, so multiplied and varied are the demands created by the artificial state of society upon the means of many of these persons, that it is not at all wonderful that so many of them find it difficult to live.

With the female part of this division of society it is worse, and more trying. But on this subject, so painful, so pregnant with disagreeable and mournful results, so much has been said of late that it is unnecessary here to recur to it.

A person in the ranks immediately above the labouring classes, but who maintains himself by the mixed species of labour, receives for his services from L.100 to L.150 annually. Upon this it is not uncommon for such an individual to support a wife and two or three children, with one servant; and with these means he supports them tolerably well. If, however, his family increase in number, his means not increasing at the same rate, he does not support them in the same comfort, and with the same facility as previously. Trusting, however, to Providence, as it is said, and often to the chapter of accidents, he continues to struggle on, and, in many respects, manfully and successfully. This, however, lasts only in the days of health and prosperity. If sickness come, and come it often does, if misfortune assail, which may also happen, the whole of this train of temporary and precarious prosperity is at once converted into a state of suffering, distress, and calamity, which it is often difficult either to conceive or adequately describe.

The common, if not the greatest evil in such cases is sickness, especially if it terminate fatally, whilst it is always most oppressive and most productive of evil, if it fall on either of the heads of the family. Severe enough, however, is it when it falls on any member of the family. Medical advice may be got often for nothing, very generally for the asking; but it is almost the only article that can be procured at this easy rate; and certainly if the worth of any article was exactly what it would bring, that is, what is paid for it, medical advice and attendance might very often be said to be absolutely of no value whatever.

Medicines must be paid for; cordials may be required; nursing and extraordinary domestic attendance must be procured; so that, all taken together, a great and unusual, and sometimes unexpected, expenditure is incurred. But if the scene be closed by the pale horse and his rider entering the mansion, these expenses are multiplied twenty, or thirty, or fifty fold. Sickness is a great calamity; want of employment is a great calamity; accidental injury is a great calamity. But of all the calamities incident, not to the sufferers but to the survivors, and to them replete with distress of every description, death is certainly the greatest and most overwhelming. And it is so, not so much by the irreparable breach which it causes, by the mournful bereavement which it occasions, but by the multiplied and aggravated expenses in which, in the present state of the usages of society, it involves the survivors.

It is no alleviation of this misery and misfortune to say, that many of these expenses are the offspring of the vanity of the living; that they are the effect of foolish, frivolous, and useless ceremonies; that they are the combined result of the fraud, cruelty, and oppression of one part of the community operating on the folly, weakness, and superstition of the other.

The multiplied and overwhelming evils resulting from this source have been long felt and known. But in the present Report they are brought out in a degree of bold and prominent relief, which must doubtless make it be matter of wonder that mankind, hungering and thirsting as they are at all times for improvement, the removal of abuses and the rectification of errors, should so long have allowed this master evil to attain the monstrous growth which it now possesses. These evils fall with different degrees of force and weight on different ranks of society. On none are they light or easily to be borne. But above all they fall with greatest force and produce the largest amount of suffering among the poor, the working classes, and those immediately above them, or those who are in the doubtful category of genteel and not wealthy.

It must be mentioned, also, that the exposition of these evils is not the sole object of the present Report. It will be remembered that we took occasion, when noticing the meritorious labours of Mr Walker in our fifty-third volume, (p. 509,) to direct attention to the pernicious practice of interring within churches, within the precincts of towns, and to the disgusting and revolting state of the burying-grounds of all large and populous cities. The present Report is devoted partly to a correct exposition of the present state of these receptacles, the multiplied evils resulting from the practice, and, as connecting with this, the enormous sums of money wasted by survivors in having the remains of a relative huddled away scarcely beneath the surface, and scantily covered, indeed, with earth, and there suffered to lie for about two years, at which time, too often, they are disturbed to make room for fresh deposits.

One of the main objects of the Report is, unquestionably, to demonstrate the injurious effects of interment within towns, and especially in churches, or in vaults beneath these edifices; and the author of the Report has certainly collected a body of evidence which we regard to be, if properly estimated and considered, quite irresistible. In conducting this argument some delicacy and address is required. Many of the facts adduced have been doubtful, and many of the arguments assigned equivocal or carried too far; and therefore they have proved nothing.

The most doubtful part of the argument, or at least that which has been most strongly controverted, is the statement that the practice of interment in towns, though represented to be injurious to the health of the inhabitants, has never been proved to be such; and it has been on the contrary asserted, that it appears from the phenomena of dissecting-rooms, slaughter-houses, glue-manufactories, tanpits, laystalls, and similar places, that the emanations from animal matters are not in any way injurious to health. In this statement there has been quite as much love of paradox as of truth; and while to the emanations of grave-yards and putrid animal matters there have been ascribed effects which certainly could not be uniformly traced to them, they have been represented as totally harmless, and without influence upon human health, upon grounds totally inadequate to justify such conclusions.

One of the most injudicious arguments, it always appeared to us, that has been employed, is the statement, that these emanations give rise to putrid, jail, or typhous fever. This is evidently not capable of being maintained; for while it is certain that typhous fever prevails in certain years as extensively and as fatally in well-aired country situations as in the dense and crowded localities of populous towns, it is also certain that between its prevalence and fatality, and the presence and exhalations of putrid animal remains, no relation can be traced. This point, indeed, we regard as decided long ago, that typhous fever is the result rather of the exhalations and secretions of living human beings than of dead animal matter.

But while we take this view of the influence and effects of dead animal matter as to typhous fever, we can by no means admit that the emanations of dead animal bodies are inert or harmless. It seems certain that they contaminate and vitiate the purity of the atmosphere; and if they do not so, to the extent and degree that might be expected, all that can be argued from that is, that the Great Creator and Preserver of the universe has provided many means of counteracting the detrimental and noxious effects of uncleanly, slovenly, disgusting practices,—whether the result of accident or carelessness,—or of the ignorance, or the folly, or the wickedness of mankind. To say that a churchyard, containing immense masses and great numbers of dead bodies putrifying, does no harm to the living neighbours, merely because they are not observed to be cut off suddenly and perceptibly by various loathsome diseases, is so contrary both to reason and experience, as well as to our unsophisticated feelings, that we think it cannot be maintained without a very passionate love of paradox. Have unambiguous experiments ever been tried on this point? and is it not sufficient to take the result of those observations that accident and other causes have instituted? It is well known that where dead bodies are congregated in great numbers to undergo decomposition, large quantities of carbonic acid gas and other deleterious gases are extricated, (see p. 5); and though these gases may not produce specific diseases, yet to breathe them cannot be said to improve the health. Farther, it cannot be denied that the inhalation of several of the gases resulting from decomposition of animal matters, besides impairing the function of respiration, very often irritates so much the alimentary canal as to induce gastro-enteritis and diarrhœa or dysentery.

"Men with shrunken figures, and the appearance of premature age, and a peculiar cadaverous aspect, have attended as witnesses to attest their own perfectly sound condition, as evidence of the salubrity of their particular occupations. Generally, however, men with robust figures and the hue of health are singled out and presented as examples of the general innocuousness of the offensive miasma generated in the process in which they are engaged. Professor Owen mentions an instance of a witness of this class, a very robust man, the keeper of a dissecting-room, who appeared to be in florid health (which however proved not to be so sound as he himself conceived), who professed perfect unconsciousness of having sustained any injury from the occupation, and there was no reason to doubt that he really was unconscious of having sustained or observed any; but it turned out, on inquiry, that he had always had the most offensive and dangerous work done by an inferior assistant; and that within his time he had no less than eight assistants, and that every one had died, and some of these had been dissected in the theatre where they had served. So, frequently, the sextons of grave-yards, who are robust men, attest the salubrity of the place; but on examining the inferiors, the grave-diggers, it appears, where there is much to do, and even in some of the new cemeteries, that as a class they are unhealthy and cadaverous, and, notwithstanding precautions, often suffer severely on re-opening graves, and that their lives are frequently cut short by the work. There are very florid and robust undertakers; but, as a class, and with all the precautions they use, they are unhealthy; and a master undertaker of considerable business in the metropolis, states, that 'in nine cases out of ten the undertaker who has much to do with the corpse is a person of cadaverous hue, and you may almost always tell him whenever you see him.' Fellmongers, tanners, or the workmen employed in the preparation of hides, have been instanced by several medical writers as a class who, being exposed to emanations from the skins when in a state of putrefaction, enjoy good health; but it appears that all the workmen are not engaged in the process when the skins are in that state, and that those of them who are, as a class, do experience the common consequences." Page 8.

The gases extricated in the course of decomposition often cause the coffins in vaults to burst; and if these coffins be not tapped in order to allow the gradual escape of the gas as it is evolved, the rupture takes place with great force and a loud explosion.

We observe that the author of the present Report has not only adopted the usual opinion, that these gases give rise to virulent and malignant fevers, and other specific diseases, but has been at some pains to adduce evidence to this effect, in the belief that the question had not been sufficiently investigated, and that the inquiry had not been carried to the necessary length. We do not think it impairs the general utility of the Report to say, that the facts and arguments adduced with this view are not adequate to establish the conclusions desired. The facts above all, drawn from the inoculation of animal matter by wounds, punctures, and scratches, are particularly inapplicable. The disease thus induced is not fever, nor typhous fever, but a peculiar disease; neither is the animal matter usually applied in this way. The only conclusion that can be justly drawn, is, that these substances vitiate the air, and cannot fail to impair the general health; but it seems doubtful that they ever induce specific diseases.

An evil of great influence in the present state of society, and in the actual condition of the lower ranks, results from the necessary delay of interments. A family, say of six, eight, or ten individuals, occupies, no uncommon case, two apartments; in some instances we have seen them in this city to be crammed into one; and we know that in London, Manchester, Birmingham, Leeds, and other populous towns, they are often packed in one single apartment. Death comes and strikes down one of the family. The dead body cannot be hastily and unceremoniously carried out and buried like the dog or the cat. It must be dressed; waked among the Irish and Scottish; the undertaker makes his visit and takes his measurements; brings the coffin; and a ceremony most important in the eyes of that class of society follows, viz. the chesting. This consists in depositing the corpse in a most solemn and impressive manner in the coffin; and while some time previously each individual is presented with a glass of wine or spirits, invariably afterwards spirits are distributed freely, and, not unfrequently, the greater part of the night is spent in drinking.

In this case what do the inmates? where do they occupy? The poor, who are kind to each other, and remarkably benevolent on such occasions, accommodate each other with their houses. On such occasions, if the house in which the death has occurred is one single apartment, all the indwellers, adult and children, are received into one or more of the adjoining houses, until the interment takes place. In some cases, as among the Irish or Scottish, where it is believed inauspicious to leave a corpse alone, the younger part of the family are sent to the neighbours, while the adults with the friends perform the ceremony of waking,—which consists in drinking as large a quantity of spirits as can be procured, talking of the good qualities of the deceased, especially his courage, spirit, and love of fun, and occasionally shedding a few tears at the remembrance of various bold, adventurous, or remarkable achievements.

In other instances, however, from various causes, the whole family are obliged to reside, to sleep, and take their meals in the same apartment with the corpse. The length of time during which a body may be retained in the dwellings of the living in this manner varies, but is always considerable. At the least it is several days or one week, (p. 35); often it is two weeks, (Bestow's evidence, p. 36); and sometimes it is continued to three weeks, (Jefferеys' evidence, p. 39.) One of the great evils ascribed to this long retention of the body, is the communication to the living of the disease by which the deceased was cut off. This is alleged very often to take place; and various instances are mentioned in which it is said that scarlet fever, *cynanche maligna*, typhous fever, small pox, and similar disorders are communicated in this manner. While we must be permitted to question the accuracy of this as a pathological fact, believing that the communication of contagious diseases from the dead body is a rare, and in several instances, if not an impossible, at least an improbable occurrence, we fully and readily admit, that the practice of retaining a slovenly and unhandsome corpse so long among the living, is not calculated to increase the comfort or improve the health of the latter. It is certainly a filthy, disgusting, and revolting practice; and the mere circumstance of sleeping and taking their meals in an apartment with a corpse, is calculated to injure health and render the living more susceptible of the attacks of sickness.

As to the apparent communication of disease in this manner, it is forgotten that, during life, while all the living processes are going on, the elaboration of the morbid poison, if such exist, is proceeding, and that then it must be most energetic; that all the inmates are more or less exposed during the day, and more exposed during the night, and that this is quite sufficient to induce the disease in the survivors, without having recourse to the dead body.

This practice is also stated very confidently to increase the mortality, (p. 37); and indeed, if thereby the sickness be increased, it follows almost necessarily that the mortality is also increased. This point, however, we care not to question further.

The evil now adverted to is of most extensive influence and most pernicious in its effects. Of course it is confined mostly to the labouring classes; and when it is known that of the deaths taking place in the metropolis, more than one-half occur among the labouring classes, and that among these four deaths among five take place in families possessing only one single apartment, it is not difficult to understand how much misery and evil this practice in all probability causes. From a table given at page 42, and taken from the Registration Tables for 1839, it appears that the number of deaths among the labouring classes are, of adults 12.045, and of children under ten years of age, 13.885, or 25.930 altogether; and consequently this practice must be carried on at least among 20,000 families annually. What a fertile source of evil, discomfort, and all sorts of mischief is here presented to the eye of the observer. In densely peopled localities there must rarely be one day, never one week, in which some of these children of affliction are not thus called on to suffer the greatest of social evils.

Among the injurious and pernicious moral effects resulting from this practice, is noticed in the present Report, the disregard, or rather the total indifference to the most impressive of all scenes on earth, that of a lifeless fellow-creature. There can be no doubt that the awe and the impressive reverence with which all of us instinctively behold the corpse of one who was lately alive and present among us, is a natural feeling, and intimately connected with all that is most excellent and laudable in the moral part of our constitution. This feeling, it is argued, however, is either rendered very obtuse or entirely obliterated by the habit of being so familiar with the object. The result is represented to be a degree of recklessness and indifference to human suffering, which, it is feared, leads to many of those atrocious and immoral acts prevalent among the labouring classes. The following extract from the evidence of a clergyman deserves attention.

> "With the upper classes, a corpse excites feelings of awe and respect; with the lower orders, in these districts, it is often treated with as little ceremony as the carcase in a butcher's shop. Nothing can exceed their desire for an imposing funeral; nothing can surpass their efforts to obtain it; but the deceased's remains share none of the reverence which this anxiety for their becoming burial would seem to indicate. The inconsistency is entirely, or at least in a great part, to be attributed to a single circumstance—that the body is never absent from their sight—eating, drinking, or sleeping, it is still by their side; mixed up with all the ordinary functions of daily life, till it becomes as familiar to them as when it lived and moved in the family circle. From familiarity it is a short step to desecration. The body, stretched out upon two chairs, is pulled about by the children, made to serve as a resting-place for any article that is in the way, and is not seldom the hiding-place for the beer-bottle or the gin if any visitor arrives inopportunely. Viewed as an outrage upon human feeling, this is bad enough; but who does not see that when the respect for the dead, that is, for the human form in its most awful stage, is gone, the whole mass of social sympathies must be weakened—perhaps blighted and destroyed? At any rate, it removes that wholesome fear of death which is the last hold upon a hardened conscience. They have gazed upon it so perpetually, they have grown so intimate with its terrors, that they no longer dread it, even when it attacks themselves, and the heart which vice has deadened to every appeal of religion is at last rendered callous to the natural instinct of fear."

The circumstances which mainly contribute to this long retention of the body above ground, are first and principally, the expense of funerals, and the consequent necessity of waiting until the necessary funds are procured by subscription, contribution, or negociation of various sorts; next, as connected with this, the delay in making arrangements for the funeral; third, reluctance in some instances to part with the remains of the deceased; and fourthly, the apprehension of premature interment.

Of all these causes the first is manifestly the most real, and that which indeed causes the greatest amount of misery and distress on the part of the survivors. It is the natural wish of all ranks, and it is not less strong among the poorer and more indigent, than among the more easy classes of society, to give the remains of their departed relative what is called, and understood to be, a decent and respectable funeral. Whatever distress and destitution the family may have endured during the life of the deceased, they make every exertion and every sacrifice to inter the remains in a decent and respectable manner. This, of course, depends not much upon the notions of the survivors. It is altogether a question of what is prescribed by use and wont; and then, of course, vanity, or to give it a less offensive denomination, the wish to appear respectable in the eyes of the neighbourhood, obliges them to comply with every observance, however absurd or unreasonable, and every ceremony, however inconvenient and expensive.

The obtaining of the necessary funds is always a serious difficulty, and the case of the death being that of the father of the family, where the duties devolve more or less completely on the widow, the distress and anxiety induced, and often the corporeal fatigue, are very great. To enable them to bear all this, spirits are often taken in considerable quantity; and the result is, that the time elapsing between death and interment is a period of mental depression, grief and anxiety, excitement from strong liquors, and great exhaustion from over-exertion, corporeal and mental.

The average expense of funerals for adults among the working classes is, according to Mr Wild, an extensive undertaker, about L. 4; for children, L. 1, 10s.; but these charges do not include ground and burial fees, which will amount to between L. 1, 10s. and L. 2 more. The difficulty of any family of the labouring classes being provided with so large a sum as this for the occasion of a funeral, has led to the formation of burial clubs and other associations, where, by paying a small sum at certain stated periods, the members become entitled, on the occasion of a death, to the payment either of the whole or the greater part of the funeral expenses as may be. In other instances, where the individual or families are not connected with these burial clubs, the undertaker is obliged to allow them time to pay; and Mr Wild states it as his general impression that, unless this indulgence were granted to two-thirds of them, they would not be able to bury their dead at all.

The club system gives rise to great abuses, and shows how much advantage is taken of the poor in their necessities. The member dying seldom leaves any money beyond the provision in his club to bury him. The widow or nominee applies to the secretary, who answers, that he cannot give any money to purchase mourning until the committee meet. This may be three months after the death. The secretary, however, who is not uncommonly an undertaker, adds, "Give me the funeral and I will advance you a few pounds on my own account;" and on this negociation the widow is obliged to submit to any charge he may make, which, it is further said, is considerable, and very often usurious, to cover all risks.

Such expenses very often cause numerous privations and great misery to families long after the interment of their deceased relative; and many of them, it is stated, never get entirely out of debt from this cause.

To those of the rank immediately above, the mischief is still more serious. There the individuals border on the genteel, and with limited, often very scanty means, are compelled, on occasions of this kind, to expend large sums of money. Mr Wild states that, to persons in the middle classes, the ordinary expenses and inconveniences of funerals are generally severely oppressive; that it often occurs that a poor widow is crippled in her means through life, by the expense of a funeral; and that an ordinary funeral, burial fees and all, will cost from L.50 to L.70, thus depriving her of L.5 annually from ten to fourteen years, besides the interest of the money expended.

"A clergyman's widow who solicited aid for her sons, whom she found it difficult to educate, states that the expenses of her husband's funeral were upwards of L.110. On being asked how she could incur such an expense, she stated that she considered it her duty to have a respectable funeral, and ordered the undertaker to provide what was respectable; that she knew not what she ordered in that condition, and merely gave general orders. And is not this a frequent case, and is not the undertaker's usual interpretation of respectability that which is expensive, the parties knowing little about it? Yes, that is frequently so."

Thus it is with this great mystery of iniquity. One of the heads of the family, father or mother, or even a leading branch, is struck down by the hand of death. The survivors are so overwhelmed with grief, that it is impossible for them to look after the requisite arrangements for interment; and even were it possible, it would not be esteemed decent. It would be thought, and even said, that they felt nothing for the death of the deceased, if they went to the undertaker and the churchwarden to arrange the interment and to check expenses. Such a cool and heartless proceeding, it would be said, was never heard of. Accordingly, the whole business is committed to the undertaker, or, as often happens, to the upholsterer, and then comes the complicated and manifold system of exaction. The upholsterer employs an undertaker, who performs the duties, and makes his charges to the upholsterer; and he, again, presents his bill to the family, or the representatives of the deceased. The undertaker charges for a funeral in this rank of life to the upholsterer at least L. 50; and when the upholsterer presents his bill to the family it is enlarged to L.100. This is what are termed second profits. And the system prevails at least in nearly two-thirds of the funerals of the upper classes, and in nearly the same proportion in the middle classes.

We have already mentioned the expense incurred at the funeral of an adult of the labouring class. The funeral of a tradesman of the lowest class, not much beyond that of a mechanic, costs from L. 10 to L. 12; of a child at least L. 5. The funeral of a tradesman of the better class, however, costs at the lowest L. 70, and may rise to L. 100; and L. 50 may be regarded as a low average for the funerals of persons of this rank; and for the funerals of children, about L. 14. The expenses seem to vary a little, also, according to the nature of the occupation of the deceased. The funeral of a curate, who is ranked as a person of moderate respectability, would cost, and does generally cost, about L. 60; that of a person in the rank of an attorney, who, we presume, must be regarded as of a little higher respectability, would cost from L. 60 to L. 100, exclusive of burial fees, tomb, or monument. When the deceased is in the rank of unequivocal gentleman, he pays still more smartly for the process of interment. Such a person cannot with decency be carried to the grave for less than L. 200, and for a sum varying from that up to L. 1000; and L. 150 must be regarded as a very low average. The interment of a child in this rank costs at an average at least L. 30.

Lastly, when the deceased is a person of rank or title—it is not said fortune—perhaps that is understood—the expense of interment varies from L.500 to L.1500; that of a child in this rank is at an average about L.50. A considerable proportion of these large sums, however, is expended in conveying the remains to a country seat, a family vault, or some similar distant situation.

In most of the provincial towns in England the expenses of interment differ little from those now stated as common in London. In Scotland the expenses of funerals of persons belonging to what are called the middling classes are stated to vary from L.12 to L.25. In Glasgow the expenses of the funerals of persons of this class are said to vary from L.12 to L.50. From various facts that have come to our knowledge, we believe that funerals of persons belonging to the middling classes are very rarely indeed performed at L.12, and much more commonly they approach to L.18 or L.20 at the least, while we have known them cost L.30, L.35, and L.40.

Much of this enormous waste of money is occasioned by the rapacity of undertakers, who have ever regarded a funeral as the most profitable department of their trade. The intrinsic value of the coffin,—materials, and workmanship—is exceedingly moderate. Yet out of this they contrive to establish a claim for L.10, L.15, or L.20, as may be. " An executor who had ordered a coffin and service of the simplest description, conformably to the intentions of the deceased, expecting the coffin to cost not more than L.5, having, under peculiar circumstances, occasion to call for the bill previous to the interment, found to his surprise that, instead of five, the charge for the coffin amounted to nearly twenty pounds. ' What,' says he, ' could be done ? We could not turn the body out of the coffin. I would have paid double rather than have disturbed the peace of the house on that solemn occasion, by a dispute, or by an objection either to that charge, or to the disgusting frippery with which those who attended the dead were covered against their tastes.' "—P. 52.

The charges, indeed, made on occasions of this kind are often in the highest degree arbitrary and unreasonable, without the slightest relation to the nature, value, or amount of the services rendered ; and they are manifestly so made in the confident belief that they are never to be questioned or checked. Any attempt of the kind would, we are informed, be ascribed to disrespect for the memory of the deceased, to want of feeling, and to twenty other reasons different from their real one. Such is the effect of slavish attention to fashion and custom ; and such the despotical authority which it may exercise over the minds of cities and nations.

Yet, with all this oppression, grasping rapacity, and heartless exaction on the part of undertakers, the business is represented to be by no means profitable or lucrative. There are, it is said, few wealthy or prosperous undertakers. By one of themselves, they are described as being some few very respectable, but " the great majority as men mostly in a small grubbing way of business." This perfectly accords with the principles of political economy. Profits on individual transactions are very large as we have seen ; but they are so much divided, that the occupation does not, of itself, afford adequate employment to one man. The whole business, indeed, is a system of licensed plunder ; and every one knows that plundering trades do not thrive. On this point a curious statement is made in the report as to the number of funerals daily for each undertaker. It appears that the number of deaths in the metropolis, excluding workhouse cases, which cannot form the subject of competition, or enter into this calculation, is, on an average of three years, 114 daily. The number of persons whose sole business is that of an undertaker, specified in the Directory for 1843 for the metropolis, is 275, which, we presume, includes undertakers and coffin makers. Besides these, there are 258 undertakers and carpenters, 34 undertakers and upholsterers, 56 undertakers and cabinet-makers, 51 undertakers and builders, 25 undertakers and appraisers, 19 undertakers and auctioneers, 7 undertakers and house agents, 3 undertakers and fancy cabinet-makers, 2 undertakers and packing-case makers, making altogether a staff of not less than 730 persons for 114 deaths, or between six or seven undertakers waiting for the chance of every private funeral. The list evidently does not comprehend the whole, and perhaps 100 or 150 might be added for those who live in bye-streets, or who are otherwise excluded. Some have two and three funerals, others eight or ten daily, so that the distribution is most unequal. Many can have no funeral for one whole week, others for two weeks, or even longer. This is probably one cause of the rapacity observed in the trade of the interment-undertaker, although many others undoubtedly concur.

The Reporter suggests that the whole business of undertaking, that is of conducting interments for the metropolis, might be better performed by 40 or 50 than even by the 275 principals, who have no other occupation, and whose establishments and expenses, as well as the cost of their own maintenance, must, if the business be equally distributed, be charged on little more than two funerals a-week.

There is indeed no doubt that the great and profitable business of interring the human race in London is, as in all other cases, confined to a few who have gotten the name and character assigned by fashion. It appears in evidence that the actual interring service is performed by about 60 tradesmen of the rank of furnishing undertakers, "who compete with each other in furnishing the orders to a multitude of inferior tradesmen, probably exceeding 1000, among whom the excessive profits arising from extortionable charges are thus irregularly distributed." It is stated further, that another evil arises out of this great abuse. All the profits do not fall to the tradesmen, though they probably obtain the lion's share. Much is spent in bribing and corrupting leading servants, and others who have, or are supposed to have influence in the disposition of a funeral. A funeral, indeed, is a concern so high, so pregnant with glorious plunder, as the watermen on the river used to say before steamers were in fashion, that to secure an order for it is a great object of solicitation, and many an undertaker would give 25 per cent. for a good one. How this beautiful system has operated, operates, and may operate, the following extract shows in no unequivocal terms.

> "Physicians of great eminence have expressed their horror at the facts of which they have been informed, of large sums of money having been promised and given to head servants to secure to the particular tradesmen the performance of the funeral. The undertakers interrogated on this subject admitted that such is an occasional, but not a universal practice; and that such sums as £10, £20, and £50 have been known to be given for such orders, according to the scale of expense and profit on the funeral. One undertaker stated that whenever a medical man took the trouble to bring him an order for a funeral, he always, as a matter of course, paid him a fee; and he believed it was a common practice. It was, however, only the inferior practitioners who brought these orders. Physicians usually carefully abstain from giving any recommendation of tradesmen in such cases." P. 54.

On this all that we say is that they must be very inferior practitioners indeed who lend themselves to practices so degrading; and any physician who did so is not worthy of the name. Is the statement not a piece of invention?

It is not without interest to observe the mode in which the large sums now mentioned for interment are expended, and the pretexts on which they are charged. Much goes for coffin as has been seen, and for burial and other fees. But decidedly the largest proportion is expended on attendants, and the observance of foolish and unbecoming ceremonies. Most persons know that the array of funerals is borrowed from the ceremonial of baronial interments; and hence all this ceremonial is imitated, so far as it can be imitated, in the most absurd way, and were it not a funeral, in a way that would merit the designation of ludicrous. The cost of porters or mutes varies from 18s. to 30s. or L.2, 2s.; and in the case where wealth is supposed, and where silk scarfs, hatbands, and gloves are supplied, the charge is exalted to the moderate sum of five guineas. For the head of the procession, who walks with a scarf, the charge will vary from one guinea to two guineas and a half. For the man who bears the plume of feathers three guineas and a half are charged. For each baton-man and each wand-bearer, L.1, 2s. must be paid; and the number of persons required for a very common funeral is 14, for one of superior rank 20 or 24. Attached to the hearse are various ornaments, as velvet and feathers, which will cost from 10 to 15 guineas. Pall, 1 to 4 guineas; clergyman, on various grounds, 2 guineas; clerk, two guineas. But to convey an adequate idea of the charges made under each head, we subjoin in a note one or two genuine undertakers' accounts.[*]

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[*] No. 1.

	£	*s.*	*d.*
Elm coffin, lined, ruffled, mattrass, sheet, and pillow	3	11	0
Leaden coffin, plate of inscription, 5 men with ditto	6	15	0
Outside case, brass engraved plate, 5 men with ditto, and making up	9	9	6
Pall 7*s.* 6*d.*, 2 porters, scarfs, staves, covers, bands and gloves, 38*s.*	2	5	6
Four gentlemen's crape scarfs, bands, and gloves	6	12	0
Seventeen silk ditto ditto	41	5	0
Hearse, 4 horses, feathers and velvets for ditto	5	16	0
Five coaches, pairs, ditto for ditto	9	15	0
Six coach cloaks, bands, and gloves, 60*s.* truncheons & wands, 6*s.*	3	6	0
Eighteen pages and bearers, silk bands, and gloves	11	14	0
Attending and assistance, 63*s.*; scarf, band, and gloves for minister, 55*s.*	5	18	0
Hat band and gloves for clerk and sexton, 30*s.*; grave digger, &c., 3*s.* 6*d.*	1	13	6
Paid vault dues 4*l.* 12*s.* 6*d.*; letters 20*s.*; fetching company 4*s.* 6*d.*	5	17	0
Two crape bands and gloves for servants 20*s.*; 8 silk do. do. 5s.	6	0	0
Thirty-four men's allowance 28*s.*	1	8	0
	£121	5	0

No. 4.

To the Executor of —— ——, Esq.

Dr to —— ——,

For the Funeral of —— ——, Esq., died 19th February, aged 80, N. 5. and 84 B., Cemetery, All Souls.

	£	*s.*	*d.*
To a 6 ft. × 22 elm coffin, lined and ruffed with fine cotton	2	10	0
Wool bed	0	10	6
Fine sheet and pillow	0	18	0
Lead coffin, solder, and workmanship	6	18	0
Lead plate of inscription	0	5	0
Inch and a half oak coffin, made to receive the above, covered with fine black cloth, 3 rows of brass nails, 4 pair of large handles, star and serpent, and finished with rays	15	15	0
Brass plate of inscription	2	8	0
To the use of the best velvet pall	0	10	6
Three crape hatbands	0	12	0
Three crape scarfs	3	0	0
Silk scarf, hatbands, and gloves, the Rev. Mr Lynarn	2	6	0
Seven silk scarfs	10	10	0
Seven silk hatbands	4	7	6
Five silk scarfs, hatbands, and gloves, Rev. Mr Rue, Mr Hawes Smith, Rule Field	11	10	0
Eleven pair of kid gloves	1	18	6
Two porters, with silk dressings	0	18	0
Two hatbands and gloves for ditto	0	15	0
The plume of ostrich feathers	1	1	0
Man carrying ditto	0	6	6
Silk hatbands and gloves for ditto	0	7	6
Hearse and four	3	10	0
Feathers and velvets for ditto	2	18	0
Three mourning coaches and four	10	10	0

The heavy pressure of these evils has led to the formation of burial clubs and similar associations, in which individuals and families, by paying periodically small sums, may become entitled to a sum adequate or nearly adequate, to defray the expense of an interment, when required. This method, however, is attended with many evils and disadvantages to the members. Often they are compelled or understood to spend a certain sum in drinking at the house where the club is held. In all instances the members pay very high for all advances made them; and often they are jobbed by the secretary of the club, being either an undertaker himself or connected with an undertaker. "The burial clubs for the labouring classes are generally got up by an undertaker, and by the publican at whose house the club is held." Thus, of an improved burial society, called the East London Burial Society, Richard Crafer is the president, the founder, and undertaker; and no future articles are to remove him so long as he gives general satisfaction to the society: and, in the event of his death, the office is to be claimed by his eldest son.

But if the poor and working classes are pillaged by these societies, the societies, it appears, are occasionally pillaged by the members. Not only adults and families, but children are entered in them, the latter, it appears, too often for the sole purpose of obtaining the burial money due on occasion of their death, and thus forming an inducement to the crime of wilful neglect, positive bad usage, and actual infanticide. A clergyman in the neighbourhood of Manchester expressed his sorrow on observing a

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	£	s.	d.
Feathers and velvets for ditto - - - - -	2	14	0
Four coachmen's cloaks - - - - - -	0	4	0
Silk hatbands and gloves for ditto - - -	1	10	0
Eight hearse pages, with truncheons - - -	1	16	0
Silk hatbands and gloves for ditto - - -	3	0	0
Six coach pages, with wands - - - - -	1	7	0
Silk hatbands and gloves for ditto - - -	2	5	0
Silk hatband and gloves for clerk at the ground - -	0	12	6
Four hatbands and gloves for servants of the two carriages	2	10	0
One hatband and gloves for terrace beadle - -	0	10	6
One hatband and gloves for man servant - - -	0	7	6
Four pair of habit gloves - - - - - -	0	12	0
Attending the funeral - - - - - -	1	1	0
Silk hatband and gloves - - - - - -	0	16	0
Twenty-six men's expenses as customary - - -	1	19	0
Turnpikes - - - - - - -	0	6	6
Paid dues at the cemetery - - - -	22	7	6
Silk scarf, hatband, and gloves (Mr Owen) - -	2	6	0
Paid for the bell - - - - - - -	0	6	6
	£130	16	0

great want of natural feeling and great apathy at the funeral; the sight of tears was unusual, and he was often shocked by a phrase among the females of the lowest class, not uncommon, "Aye, aye, that child will not live; it is in the burial club."

It appears that in Manchester the actual cost of the funeral of a child is from L.1 to L.1, 10. But the clubs allow nearly L.3, and in some L.4 and L.5. Insurances against burials are made in three or four clubs, and the result is, that in order to obtain the money thus gambled for, the life of the child is sacrificed. It is well known that two cases fell under the notice of Mr Coppock, the clerk and registrar of the Stockport Union, in both of which the circumstances led him to suspect that three children had been poisoned with arsenic. He accordingly prosecuted the parties for murder. In one case the father was convicted and the mother acquitted. In the other case the father was acquitted. In all the bodies arsenic was found on inspection and chemical analysis. Here is disclosed a shocking source of demoralization and wickedness.

It appears, that, for all the funerals in England and Wales, the sum of L.4,871,497 is expended annually. Of this enormous sum thus annually thrown into the earth, it appears that a large proportion might be saved, more especially for the funerals of the middle and higher classes; and the reporter observes that even in the metropolis alone the waste on funerals would, in a short time, suffice for the foundation of educational and similar institutions, by which the condition of the poorer classes might be materially improved. "The waste of two years in the metropolis would suffice for the erection of a magnificent cathedral, and of a third year, for its endowment for ever."

As he advances in his Report, he notices the necessary evils of having burying-grounds in the midst of cities, in the centre of crowded thoroughfares, and all the manifold evils thereby resulting; obstruction to the procession or the ceremonies;—interruption or obstruction by noise, by incongruous sounds;—two, three or four funerals coming all, or nearly all, within a short time of each other; and in short, every one of those revolting and indecent exhibitions at, or in the course of funerals with which the inhabitants of large and populous towns are daily offended.

The great point is the remedy or remedies. And first, as to the long retention of the body unburied, and the main alleged cause, the dread of premature interment, the reporter suggests the adoption of means similar to those which have been long in use in Munich, Frankfort, and other foreign towns. In this respect we are certainly very far behind our neighbours on the Continent. It is well known that 52 years ago Hufeland directed the attention of the municipal authorities of Weimar to this subject; and since that time the provision by the public of means for obviating those evils has been very generally adopted in various parts of Germany.[*] For the lower classes it would be most important and beneficial to establish, in convenient localities, proper and well-ventilated reception houses or funeratories, to which the remains of their friends might be conveyed, and in which they might remain from the period of death to that of interment. Even in the present state of matters something similar to this has been long in use in London. Bodies are often conveyed to the premises of undertakers, and there allowed to remain until interment; and the friends at that time attend the body thence to the burial-ground. This has arisen out of various causes. Sometimes it has been the effect of the want of accommodation in the house of the deceased. Sometimes it has been the result of the individual dying in a hotel or lodgings from which the proprietors were anxious to have the body removed. An arrangement of local funeratories for each district or parish would be, if placed under public protection and inspection, in some circumstances invaluable, and might be the means occasionally of detecting the causes of sudden deaths, either mistaken for murder or suicide, or actually of detecting cases of murder and suicide when alleged to be accidental or the result of disease.

The main object evidently must be to reduce the amount of these expenses to all classes, but especially to the labouring ranks, and those immediately above them. Such reduction, we are told, would be a great and most important relief in numerous cases of widowhood and similar destitution, where the widow or the surviving children incur debts, which not unfrequently hang over them for life, and throw them on the poor rates. "It forms a large part of the business of some of the small-debt courts in the metropolis, to enforce payment of undertakers' bills incurred under such circumstances." And it is afterwards stated, that, "by the arrangements which throw the savings of the poor family into the grave, children are left destitute, and creditors are often defrauded, and heavy taxes are levied on the sympathies of neighbours and friends."—P. 78.

The worst and most pernicious part of the whole system, is that, while the expensive and foolish decorations are borrowed and imitated from the practice of the middle ages, and the observances and ceremonies of knighthood, they present no relation either to the genuine grief felt by the mourners, or to those religious and moral sentiments with which we ought to part with the mortal remains of our departed friends. Viewed with the calm eye of reason and religion, nothing can be more inconsistent with the principles of Christianity, nothing more unbecoming what ought, we know, to be its practice, than the ceremonial and magnificence, real or only imitated, of Christian interments in this country. A procession partly military, partly baronial; customs and observances adopted from the chivalry of the dark ages; a barbaric display of the pomp, pride, and circumstance of the Norman-Gothic manners, are strange accompaniments to the removal of the remains of the Christian believer to their last earthly abode. Yet such is the fact; and for the accomplishment of such purposes immense sums of money are daily and weekly thrown into the earth. All is the gratification of pride or vanity, the weakest, if not the worst of the human passions.

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[*] Ueber die Ungewissheit des Todes, und das einzige untrugliche Mittel sich von seiner Wirklichkeit zu uberzeugen, und das Lebendigbegraben unmoglich zu machen; nebst der Nachricht von der Errichtung eines Leichenhauses in Weimar. Kleine Med. Schriften Band. i. p. 272.

From the testimony of various respectable undertakers, it seems quite undoubted that very great reductions might be effected; more perhaps in the funerals of the more wealthy classes than in those of the lower classes in the scale of society. Still there is good evidence that even there much saving might be effected, and much destitution and oppression prevented. Mr Wild is of opinion that a reduction of 50 per cent. might be effected, without impairing what is regarded as essential to the respectability to the funeral. The whole expense of a funeral, according to this gentleman, would, on such a reduced scale, vary in the case of an adult from L.3, 1s. to L.10, 8s. for trades-people, and in the case of a child, from L.2, 6s. to L.4, 12s.; for mechanics, from L.2, 4s. to L.3, 1s. for an adult, and from L.1, 5s. to L.2, 6s. for children.

Other reductions might result from different arrangements. Thus, as the effect of the crowded state of all the metropolitan and parochial burying grounds, various cemeteries, (8 at least,) have been instituted in the suburban districts. In some instances the expenses seem to be increased rather than diminished by conveying the dead to these cemeteries, which are managed mostly as joint-stock concerns, and consequently money-making speculations, and which are always at a considerable distance from the most central and populous parts of the city. Various reductions, however, might be effected by forming national cemeteries, or cemeteries provided by the public, where the expense of maintenance and keeping in order would be the sole expenditure. By diminishing the number of mourners and attendants, by conveying the body in a light hearse, decent, but without ornaments, and by having a body of bearers employed, and governed by a public board, or some authorized body, it appears that very considerable savings might be effected. It appears that there is prevalent among the working classes a strong feeling against the parochial burying-grounds, manifestly from their crowded state, and the careless and offensive way in which funerals in them are too frequently conducted; and a feeling not less strong in favour of interment in the cemeteries, notwithstanding that often it is there more expensive than in the parochial grounds. Much of the expense, Mr Chadwick thinks, might be greatly diminished by having one or more national cemeteries, not on the joint-stock principle, but under public protection and authority. It appears that there are in the metropolis eight chief cemeteries by private companies, embracing a space of 260 acres, which is considerably more than the space occupied by all the parochial and private burying-grounds. On these a capital of about L.400,000 has been expended; and the establishment charges are stated to be about L.7500 per annum. The original cost, Mr Chadwick thinks, is much greater than by proper management it might be; and he infers, that for an amount not much greater, or not exceeding it by more than one-fifth, superior national cemeteries, with houses of reception, and suitable chapels, might be formed on the present scale of expenditure of these companies, and in a style commensurate with what is due to the metropolis of the empire.

One difficulty which will occur to every one, the necessity of providing means for the performance of the funeral service, the reporter obviates in the following manner. He proposes four national cemeteries, each with a chapel attached; and assuming that the greatest solemnity, and the full cathedral service is due to funerals, he infers that four full choirs of twenty choristers, each led by it own organist, might be obtained for less than L.10,000 per annum.

He next proposes a body of officers of health, who might have the power of visiting houses in which deaths took place, reporting them and their causes as near as can be conjectured, and giving the requisite directions for the interment of the deceased. For this duty he suggests one inspector of public health, with pay at the rate of L.1, 16s. per day; one deputy-inspector, with pay at the rate of L.1, 4s. per day; eight inspectors at 19s. per day; two supernumeraries at 15s. per day; and ten single horse vehicles, and ten drivers, at L.1, 1s. per week. Ten officers of the kind now suggested, visiting fifteen cases daily, would suffice to take orders for the burial of 45,000 persons; and to perform, besides, to the survivors various other duties which medical officers alone can properly perform. The total expense of this staff amounts to L.6054, 10s. per annum.

Another evil arises which we should like to see rectified, or at least put on another footing. In the case of the clergymen of the Established Church in large towns, the surplice fees, including the burial dues, are, we are told, to be considered as the main parts of their income. They have no better and no other means of livelihood. This, therefore, it is argued, requires compensation; and as the burial dues are variously regulated, and differ in every different parish, it becomes impossible to lay down any general rule. Mr Chadwick arrives, therefore, at the conclusion, that the only satisfactory mode of determining the amount of compensation would be an adjudication and examination in the case of each parish.

From this plan of procedure we totally dissent. We would propose that the whole root of the evil should be gone into and completely removed. All surplice fees, and all burial fees, forming part of the regular and necessary income of the clergyman, we regard as radically bad, so very bad that nothing but long established custom and practice can ever think or say any thing in its defence. It is no argument to say that the clergymen have no tithes and no other means of livelihood. If they have not, it is full time that they have, and that suitable and ample provision should be made for maintaining in comfort and respectability, a body of men whose utility is undoubted, from a source totally different. Let a regular assessment rather be laid on the parish, not large, but sufficient to answer the purpose, and let pew rents, and other sums be, as far as circumstances will admit, applied to the same purpose; and surely an adequate income for the clergyman might be provided, without taxing such an offensive source as that of surplice fees and burial fees. We believe that more ill blood and bad feeling has been engendered against the clergy of the Established Church by this one circumstance, than by many others taken together. It places the clergyman in a false position in relation to his parish.

Mr Chadwick is a great compensator in his way, and we believe that his propensities in this respect originate in truly benevolent feelings. We admit the necessity, not of compensation for loss to parochial clergymen, but of placing their revenues on a more stable and more becoming foundation;—in short, of altering the system entirely. If there is one part wrong in Mr Chadwick's proposition, it is, that he cannot get out of the trammels of this besetting sin of compensation. He, accordingly, after having made what he regards a good proposition in one case, but what we maintain is a most discreditable and pernicious one, regarding the means of compensating clergymen, proposes next to compensate undertakers, or "tradesmen who have devoted themselves entirely to the business of supplying funeral materials and service, and who will be wholly superseded." As soon would we recommend compensation to stock-jobbers, on occasion of any change in the business of funding which would render these persons unnecessary, to gamblers, and gambling-house keepers, on shutting up six of the most fashionable hells in the metropolis, or to betters on the Derby,—if horse-racing should ever be suddenly put down by Act of Parliament. Nothing can be more preposterous than to offer any premium, or give any rewards to the labours of such persons as undertakers, if they should, by any measure deemed requisite for the public benefit, be deprived of the great mass of their profits. If they have hitherto proceeded successfully in extorting money from all ranks of the community at the most trying of all times, that constitutes no reason for any continuance of their gains, far less any compensation. They plunged into a trade, which it is not unjust to designate as usurious, with their eyes open, exactly as stock-jobbers, gamblers, and other adventurers; and if they have not succeeded, that is not the affair of the public. We hope and trust, indeed, for the sake of example, if nothing else, that no such plan will ever be entertained.

To avoid the collision of different religious parties, the author properly and judiciously proposes to place the whole arrangements under the superintendence of the Commissioners of Woods and Forests.

On one subject by no means easy, the collection of the necessary funds from the poorer classes, different plans are suggested. The author is most partial to the plan of levying such charges in the manner of a burial club; *i. e.* something in the way of a poll-tax or burial dues, payable per head, or the number of persons inhabiting each house. Some measure of this kind must be employed so long as the working classes are either so destitute or so improvident as not to save money for emergencies like funerals. On some of the circumstances relating to this part of the subject, the following extract deserves serious attention; and we recommend it, because we believe that several of the evils therein adverted to might be averted, or at least diminished, by attending to the suggestions recommended.

"234. It is, however, to be borne in mind that in burial clubs, and in savings' banks, large sums are now actually set apart by the labouring classes for the payment of funeral charges. Provision is, no doubt, also made by will, by other classes for defraying such charges. In the plan proposed, even including the expense of the new agency of officers of health the consideration of new sources of additional payments is rendered unnecessary. On the whole, therefore (although if bodies are immediately removed from the premises in cases where the removal is requisite for the protection of the lives of the survivors, attempts will be made to shift the expense to the public), it may be recommended that all new charges and compensations should, for the present, at least, still be defrayed from burial dues levied upon each interment. And in so far as any new expenses are for objects obviously beneficial (not to speak of those immediate charges being for the most efficient means of reducing the aggregate expenses), it will meet with ready acquiescence. I have consulted intelligent persons of the labouring classes, and discussed with them step by step the proposed changes. They have unanimously declared that these changes would all be a great gain to them, especially the proposed reduction of the expenses of interments. They have moreover urged that if they were enabled to have the funerals performed in a satisfactory manner, at a reduced expense, the applications for parochial aid would be proportionately diminished, the poorest relations would then subscribe to avert the disgrace of a parochial interment; a large proportion of the applications for such aid being now made by others than regular paupers, and in consequence of the hoplessness of their being enabled to defray the heavy expenses which are at present necessary."—Pp. 194-5.

The whole of the changes proposed are presented in the following summary.

"§ 248. That the most effectual and principal means for the abatement of the evils of interments are those sanatory measures which diminish the proportionate numbers of deaths and funerals, and increase the duration of life. § 75 to § 82, and General Report, p. 370. But—

"§ 249. That on the several special grounds, moral, religious, and physical, and in conformity to the best usages and authorities of primitive Christianity, § 177, and the general practice of the most civilized modern nations, the practice of interments in towns in burial places amidst the habitations of the living, and the practice of interment in churches, ought for the future, and without any exception of places, or acceptation of persons, to be entirely prohibited. (§ 1 to § 23.)

"§ 250. That the necessities of no class of the population in respect to burial ought to be abandoned as sources of private emolument to commercial associations, but that national cemeteries of a suitable description ought to be provided and maintained (as to the material arrangements), under the direction of officers duly qualified for the care of the public health. (§ 126.)

"§ 251. That for the avoidance of the pain, and moral and physical evil arising from the prolonged retention of the body in the rooms occupied by the living, and at the same time to carry out such arrangements as may remove the painful apprehensions of premature interments, institutions of houses for the immediate reception, and respectful and appropriate care of the dead, under superior and responsible officers, should be provided in every town for the use of all classes of the community. (§ 90 to § 101.)

"§ 252. That for the abatement of oppressive charges for funeral materials, decorations, and services, provision should be made (in conformity to successful examples abroad) by the officers having charge of the national cemeteries, for the supply of the requisite materials and services, securing to all classes, but especially to the poor, the means of respectable interment, at reduced and moderate prices, suitable to the station of the deceased, and the condition of the survivors. (§ 186, § 115 to § 120.)

"§ 253. That for these purposes, and for carrying out the physical arrangements necessary for the protection of the public health in respect to the practice of interment, officers of health qualified by medical education and special knowledge should be appointed. (§ 223.)

"§ 254. That in order to abate the apprehensions of premature interment, § 92 to § 96, to bring responsible aid and counsel, and protection within the reach of the most destitute survivors, § 121 and 122 and § 198, to protect the people against continued exposure to ascertained and preventible causes of disease and death, the principle of the early appointment of searchers be revived, and no interment be allowed to take place without the verification of the fact and cause of death by the officer of health. (§ 123, 124, 125, 126 to § 216).

"§ 255. That in all clear and well ascertained cases of deaths from immediately removable causes of disease and death, the officers of health be invested with summary powers, and be responsible for exercising them, for the removal of those causes, and for the protection of strangers from continued exposure and suffering from them.

"§ 256. That the expenses of national cemeteries should be raised by loans bearing interest.

"§ 257. That the repayment of the principal and interest should be spread over a period of [thirty years?]—and be charged as part of the reduced expenses for future interments.

"§ 258. That all burial fees and existing dues be collected on interment, and form a fund from whence be paid the compensations which Parliament may award to such existing interests as it may be necessary to disturb, including the payment of the establishment charges, and the principal and interest of the money expended for the erection of new cemeteries; and that any surplus which may thereafter accrue may be applied to the means of improving the health of the living.

"§ 259. That on consulting the experience of those cities abroad where the greatest attention has been given to the arrangements for the protection of health connected with interments, it appears that by the appointment of medical officers, unencumbered by private practice, as officers of health, and qualified by the possession of appropriate science for the verification of the fact and causes of death, and by committing to them the regulation of the service of interments in national cemeteries, the several defects above specified may be remedied, and that new and comparatively salubrious places of burial may be procured, together with appropriate religious establishments, wherein the funeral service may be better solemnized, and that the expense of funerals may be reduced in the metropolis, at the least, to one-half of the existing amount, and full compensation be given to all who may have legitimate claims for compensation for losses on the alterations of the existing practice. (§ 219 to § 225.)

"§ 260. That the agency of properly qualified officers of health necessary for abating the evils of the practice of interments would also serve powerfully to promote the application of those sanatory measures which in some districts would, there is reason to believe, save more than their own pecuniary expense, merely in the diminished numbers combined with reduced expenses of funerals, consequent on the practical operation of comprehensive measures of sanatory improvement. (§ 201.)"

We have only to say that this Report is highly creditable, not only to the activity and industry, but also to the feelings of Mr Chadwick. No one but a person of great benevolence as well as active benevolence, could have undergone the labour which he has done in the cause. The evils were long known in a general way and lamented; but they could never have been brought forward in the accurate and distinct manner in which they are in the present Report without the greatest labour and care, and without that sort of enthusiastic attachment to the subject which converts labour into pleasure, and renders the task of overcoming difficulties a source of enjoyment and happiness.

ART. III.—*The Anatomy, Physiology, and Pathology of the Human Teeth, with the most approved methods of Treatment; including Operations, and the method of making and setting Artificial Teeth. With thirty plates.* By PAUL B. GODDARD, M. D., M. A. N. S., M. A. P. S., Demonstrator of Anatomy in the University of Pennsylvania, &c. &c. *Aided in the Practical Part* by JOSEPH E. PARKER, Dentist. 4to. Philadelphia, 1844. Pp. 227.

WE do not possess a modern work on Dental Surgery, written by a British author, which equals that of Dr Goddard. One reason for this may arise from the circumstance, that the learned author is a practical anatomist, whose knowledge is on a level with the modern discoveries, and who has himself authenticated the latest researches into the minute anatomy of the dental structures. It is quite apparent that such knowledge must prove of immense value in enabling any one to arrive at just conclusions relative to the diseases of the teeth; and it is chiefly to be attributed to the want of such knowledge that most writers on Dental Surgery have erred so much relative to the causes and nature of these diseases. In our fifty-third volume, (January 1840;) and also in our fifty-fifth volume, (January 1841,) we were at considerable pains to point out the proper position which the dental structures ought to occupy in an anatomical classification; and endeavoured to prove, from their mode of development, their structure and their diseases, that they ought to be arranged in the same category as the nails, claws, spines, hair, feathers, and other epidermic appendages. The necessary corollary from this conclusion was, that the diseases to which they were liable did not depend on an internal vital action, similar to inflammation and its consequences in other tissues, but resulted from a chemical action of vitiated solids or fluids in contact with them, or from disease of the parts in which they were fixed. Every new fact which has been added to our knowledge of these structures since then has tended more and more clearly to demonstrate that the conclusions we then arrived at were the correct ones; and Dr Goddard in his present work has taken the same view of the question.

To the readers of this Journal it would be a work of supererogation to enter into the minute details of the anatomical or physiological part of the volume before us, seeing that in the articles to which we referred above, and in several other papers which appeared lately in our pages, are contained all the information which we could extract from Dr Goddard's work. That which will give it its chief value to the dentists of this country, viz. the minute accounts of the various processes for the filing, stuffing, extracting, and supplying the loss of teeth, together with the mode of preparing artificial enamel, or porcelain teeth, comes not within the range of this Journal; yet to all interested in such matters, the work may be confidently recommended, as containing the best and most approved methods of performing all the operations connected with Dental Surgery.

The work is divided into six parts. The *first* includes the History of the Teeth, and general consideration as to the jaws, alveoli, &c. The *second* part treats of the Anatomy of the teeth. The *third*, of their Physiology. The *fourth*, of Dental Pathology, Surgery, and Therapeutics. The *fifth*, of Dental Hygiene. And the *sixth* part, of Mechanical Dentistry.

In speaking of the articulation of the teeth, Dr Goddard describes as an important structure, which has hitherto been too much overlooked, a fibro-ligamentous band which exists at the margin of the alveolus, and unites it closely with the neck of the tooth. This structure, which he described several years ago in the American Journal of Medical Sciences, he named the *ligamentum dentis*. It is very feebly developed, or altogether wanting in the incisor teeth, but is so strongly developed around the bicuspids and molars, particularly where they are contiguous, that it presents a serious obstacle to their extraction, unless previously divided. It is probably in consequence of the firm attachment of this ligament to the edge of the alveolus that a portion of the osseous margin is generally removed along with the molar teeth when they are extracted. From his description it would seem as if this elastic fibro-ligamentous structure were the chief agent concerned in preventing the teeth, when masticating, from pressing injuriously on the alveolar cavities, or sensitive pulps of the teeth. He states that this structure allows a slight degree of motion when the tooth is grasped firmly by the finger and thumb, and moved—a movement which is not attended by any painful sensation when the pulp is in a healthy state. In confirmation of his opinion, that the teeth do move very slightly during mastication, he states that if two molars which touch each other be examined at their points of contact, it is always found that the enamel at these points is considerably worn. We have several times observed the same fact, but never adverted to the probable cause, which, however, seems sufficiently accounted for by Dr Goddard.

Though three and a half years have now elapsed since we endeavoured to draw the attention of anatomists and dentists to the probable mechanism by which the temporary set of teeth were removed, we do not find that any distinct researches have been made on the subject. Even Dr Goddard, who seems to have investigated personally all the other facts relative to the teeth, seems in this case to have given an opinion as to what was likely. From having arrived, however, at the proper conclusion relative to their vitality, if we may so speak, he very properly attributes the absorption of the fang to the agency of the surrounding parts. This, however, leaves the question exactly where it was before we took up the subject in 1840, and showed that the observation of Delabarre, as to the chief agent of absorption being a fleshy button formed by a transmutation of the external membrane of the follicle of the permanent tooth, was the only explanation which corresponded with the facts we had frequently observed. If the death of the milk set depended on the pressure exerted by the permanent set, this change should take place, and the fang of the milk tooth be destroyed, whether the bony partition which exists between the two sets had been destroyed or not. But all are familiar with the fact noticed by Hunter, Fox, &c. that if this osseous partition be not destroyed, the milk tooth does not drop out, but remains as a permanent tooth; and it is possibly to this circumstance we are to ascribe the fact mentioned by Ploucquet and others, of their having seen no fewer than five permanent incisors in the lower jaw. All, on the other hand, who have attended to the anatomy of the jaws, must be familiar with the fact, that when a milk tooth drops out from absorption of its fang, a vascular fleshy button is found below it,—an organ which must have been the immediate agent in producing the absorption of the fang.

There is only one other remark we would notice in Dr Goddard's work, and it relates to the causes of the decay or caries of the teeth, as being an additional and strong corroboration of the opinions we have long advocated, as to that disease depending on an external or chemical action, but not the result of a vital process. When human teeth are placed in the mouth to supply the place of those which are lost, "they decay," says Dr Goddard, "as readily as the living tooth, and the progress of the caries is just as regular. It pursues the same course, and with the same rapidity; thus furnishing a strong argument in favour of the views of decay taken in this volume."

We cannot close our remarks without adverting to the thirty very beautiful lithographs which illustrate the text. They render it quite impossible to misunderstand the author, and afford a very favourable example of the advanced state of the art in the American continent.

ART. IV.—*Lectures on the Principles and Practice of Physic.* Delivered at King's College, London. By THOMAS WATSON, M.D., Fellow of the Royal College of Physicians, Physician to the Middlesex Hospital, &c. London, 1843. Vol. i. p. 832, Vol. ii. p. 812.

THE present work comes before the profession with very strong claims upon their particular attention; and we have no doubt that it will by its great intrinsic merits speedily realize the expectations which we now express. It is the production of a physician of undoubted talent and great learning, and whose industry in performing the most laborious duties of his profession has been well known now for a long series of years. As physician to a large metropolitan hospital, Dr Watson has not only enjoyed great advantages in cultivating the knowledge of the practical part of his profession, but of these advantages he has availed himself with great diligence and judgment; and he has accordingly produced a work which combines, with correct knowledge of scientific principles, all those elucidations which observation and experience alone can furnish, and without which any work on the practice of medicine would be meagre, incomplete, useless, and probably unsafe.

The work, strictly speaking, cannot be regarded as quite new; for the lectures, of which it consists, appeared, most of our readers are aware, in successive numbers of the London Medical Gazette, and consequently great part of the work may be said to have been before the public fully two years previous to its appearance as a separate publication. As, however, it is always most just to form a judgment of a work taken altogether, and when all its parts are completely before the public, it can be only at the present time, that it is seasonable to speak of its contents and their character, when such a judgment can be adequately formed.

Another circumstance requires consideration in forming an estimate of the present work. It appeared at first in the form of lectures which the author delivered, as professor of the practice of medicine, to his pupils in King's College; and this form Dr Watson has strictly retained. This invests the work with something of the rhetorical character, and renders the individual discourses perhaps more full and prolix than they would have been in the case of a mere system. We do not, however, say that it renders the work thereby less didactic or less interesting. It is perhaps more so. But it nevertheless throws over the whole work more of the air of the spoken discourse than of the condensed systematic treatise, and besides that, it is liable to render the method of arrangement more forced and artificial than it would otherwise be. The arrangement of any subject, indeed, in pronounced discourses, must be, even in the most dexterous and accomplished hands, more or less artificial; the lecture must be delivered within a given period of time; and if this period of time is either too long or too short for each subject, the subjects are necessarily divided; and the most skilful management cannot render such a division in all cases natural. To-day one subject must be left off in the middle, in order to be resumed at next lecture. To-morrow the discussion of another subject is completed before the time is exhausted; and the rest of the hour is occupied in the partial examination of a different subject. Then the lecturer may be tempted occasionally to shorten or prolong the subject, according to the measure of the time. These are reasons that perhaps would have had weight in causing some to wish that the work had been thrown into another form, in which the appearance of the spoken discourse would have had, or seemed to have, less influence in determining the general character of the work. We think it not at all unlikely that many would have wished the materials taken out of the shape of lectures and arranged more on the principles now referred to.

Let us not, however, allow the one side only of the question to be viewed. It cannot be denied that the shape of lectures, that is, discourses composed and spoken in a certain order, has also its advantages; and that these advantages are in many cases neither few nor unimportant. In the first place, it gives to the work a species of animation and life, for which we often look in vain in the formal systematic treatise. If also the lecturer is occasionally betrayed into a colloquial manner, that certainly has its advantage in this respect, that often it is more impressive and warmly interesting. Thirdly, all the illustrations are also given with more effect in the form of lecture than in any other. Besides these reasons there occur often others applicable to the pupils, or the lecturer, or to both. Between them a temporary relation is created; and all of us know how strong an impression truths and principles make conveyed by a particular speaker, and delivered under particular circumstances. Something like this, we believe, may have been felt by the pupils of Dr Watson; for we are told that it was at the request of many who had heard these lectures that they are now published in a collected form.

These observations we offer, not with the view of censuring, or even criticising the present work, but in order to explain some of its peculiarities; and when we have so done, we conceive that it is unnecessary to do more than give a brief account of the general contents of the volume, and point out, as far as we may be able, its peculiar merits.

The arrangement of the materials is simple, but quite as scientific as the present state of medical science will allow it to be. After an introductory lecture on the distinctions between health and disease, and the general nature of diseases and morbid diatheses, Dr Watson gives a short sketch of the mode in which he proposes to treat the subject. He then explains the mode of unfolding the principles and practice of medicine, and the advantages of teaching them by lectures; and concludes with some just and impressive observations on the high responsibilities of the medical practitioner, and how much he ought to feel the necessity of rendering himself equal to those responsibilities. The second lecture is devoted to the explanation of the term Pathology, and the characteristic distinctions between General and Special Pathology; and in the subsequent part of this and the six following lectures, the author gives a useful and instructive view of the former department of medical science, under the following heads: I. Changes in the solids, as Hypertrophy and Atrophy; Induration, Softening; Transformations of Tissues; New Tissues; Changes in Situation. II. Morbid Alterations of the Fluids, especially the blood; Plethora, General and Local; Congestion, &c. III. Different modes in which Death takes place; Sudden Death; Death by Anæmia; by Asthenia; by Syncope; by Inanition; by Apnœa; and by Coma. IV. Causes of Disease, and appreciation of their influence: and, V. Symptoms, and the sources from which they are derived.

In the ninth and tenth lectures, Inflammation, its phenomena, its pathology, and its symptoms and effects are considered. In the eleventh lecture the subjects considered are Mortification as an event of inflammation; Inflammatory Fever, Hectic Fever, and

Typhoid Fever; the influence of the difference of Tissues in modifying the phenomena of inflammation. In the twelfth lecture are considered various forms of Inflammation; and, in connection with struma, the history of the development of tubercles. In the thirteenth lecture are considered Cancer and its various forms; and the close of this lecture and the whole of the fourteenth are devoted to the Treatment of Inflammation.

In the fifteenth lecture the author treats of Hæmorrhages, their pathology and treatment; and in the sixteenth of Dropsy, its causes, varieties, pathology, and treatment.

Hitherto, the work may be said to be devoted chiefly to the elucidation of the principles of general pathology. After this the author enters on that division which has been named Special Pathology, the divisions of which are treated in the following order:

Four lectures, viz. the seventeenth, eighteenth, nineteenth, and twentieth are devoted to a very full and correct history of the Diseases of the Eye, and the treatment appropriated to each.

From the twenty-first to the thirty-ninth lectures, inclusive, that is, in the course of nineteen lectures, Dr Watson gives a history of all the disorders and diseases of that extensive and complicated part of the animal frame called the Nervous System. This is done with great ability and judgment, and in a manner which every one must regard as at once instructive and interesting. The limits of this article do not allow us to enter into details, or illustrate our general inferences by extracts; but we may say without exaggeration, that we have rarely found articles written with such ability and interest as those on *Delirium Tremens*, Hydrophobia, and Hysteria in the present volume.

In the three subsequent lectures the history of Intermittent Fever, its varieties, its pathology, and its treatment, are delivered at great length and with much interest.

The author then proceeds, much in the manner of the usual anatomical arrangements, to consider various diseases taking place in the head and neck; as epistaxis, bronchocele, cretinism, *cynanche parotidæa*, and *cynanche tonsillaris; cynanche laryngea*, and its treatment in detail; *cynanche trachealis*, and crowing inspiration.

With these subjects in the forty-sixth lecture the first volume is concluded.

In the second volume, the first eighteen lectures, viz. from the 47th to the 64th inclusive, are devoted to the diseases of the thorax, including under this name the diseases of the lungs, the diseases of the heart, those of the aorta and veins, and diseases of the œsophagus. The first of these lectures, viz. the 47th, is devoted to the method of examination, and the subjects of auscultation and percussion, and the recognition and apprciation of symptoms and signs. The merits of the rest of these lectures it is not easy to particularize. Correct and perspicuous accounts of the different diseases are given; and the treatment respectively applicable to each is judiciously and clearly explained.

We are pleased to see some judicious observations on several of the stethoscopic signs, both in speaking of pneumonia and also of phthisis.

Next follow the diseases of the abdomen, which are amply considered in eleven lectures and the first part of a twelfth. They are considered in the following order:—Peritonitis, in its different forms, ascites, ovarian dropsy, gastritis, hæmatemesis, and melæna, dyspepsia, enteritis, colic, diarrhœa, dysentery, intestinal concretions, worms, with the question of their formation, diseases of the liver, jaundice, diseases of the gall-bladder, of the spleen, and pancreas.

The latter part of the 76th lecture, and the whole of the four succeeding lectures, is occupied in explaining the history, causes, pathology, and treatment of the diseases of the kidneys and the morbid states of the urinary secretion; and at the end of the 80th lecture a short general account of the nature of abdominal tumours, some of them not referrible to any of the previous heads, is given.

Rheumatism, acute and chronic, and gout in its different forms, are treated in the 81st and 82d lectures; and in the eight following discourses, including the 90th, which is the last, a short but very useful history of the cutaneous diseases, and their most established modes of treatment, is placed before the reader.

Such is a short view of the contents of this work, and the manner in which they are arranged. Already have we said, that our limits will not permit us to enter into the detailed examination of any of the subjects; and all that we can here do, is to mention our opinion generally from the impression left by perusal of the work.

When, therefore, we say that Dr Watson has with great judgment selected the most important and useful parts of each subject; that he has given correct and faithful views both on the symptoms and the pathology of the various diseases; and that he shows much judgment in explaining the therapeutic principles and rules, which should guide the physician in conducting the treatment,—we feel that we say not more than the perusal of the volumes by any candid and well-informed judge will most amply confirm. Let us not forget to add, that the style and general character of the work is peculiarly practical; and the cases which Dr Watson has from time to time introduced to illustrate his views, are highly appropriate and interesting, and add much to the value of the work. And this certainly must be admitted to be one of the great advantages of casting this work in the shape of lectures, in which these cases assuredly appear more fitly, and in which they are introduced more easily and naturally than they could have been, had the form of the work been different.

Lastly, we are well pleased to observe that a strong vein of common sense, as well as good taste, runs through the whole treatise, and sustains both the interest and the confidence of the reader throughout. Often have we had occasion to lament that physicians and surgeons, in bringing out the scientific principles of their respective arts, seemed to lose sight entirely of this precious guide, and, consequently, allowed all sorts of wild and absurd methods of treatment to occupy an important place in their estimation, or places much more important than they ought to do. Such men seem to forget altogether that man is mortal; that a certain number of diseases are, and must be, from their nature, wholly unmanageable; and that the number of remedies, properly so called, with which essential benefit can be effected in curable diseases, is very small indeed. The great duty of the judicious physician, we conceive, is to teach his pupils these facts, as well as to communicate to them the general principles of the art of healing; and any one who neglects to do so performs only half his duty. The present work, we think, shows the value of these observations in a strong and clear light. Dr Watson has always endeavoured to show how far the practitioner ought to go in treatment, and what is the proper treatment, and sets himself firmly against all therapeutic measures which cannot be recommended by common sense and experience.

ART. V.—1. *A Manual of Medical Jurisprudence.* By ALFRED S. TAYLOR, Lecturer on Medical Jurisprudence and Chemistry in Guy's Hospital. London, 1844. 12mo. Pp. 679.

2. *Principles of Forensic Medicine.* By WILLIAM A. GUY, M. B., Cantab., Professor of Forensic Medicine in King's College, London. Parts I. and II. London, 1843. 12mo. Pp. 380.

THE current of medical publication runs most strongly and abundantly at present in what may be called the manual way. Here are two publications, one completed, the other, two-thirds done, we are assured, both appearing in the manual form. It must not be understood, nevertheless, that by the term manual is meant at present exactly the same thing as was meant twenty or thirty years ago. It is not as then the mere essential elements of any given department of science, presented in a small, compressed, and portable shape. By having recourse to very small type, of this peculiarity the manual is completely deprived, and it is only in external characters, not in internal contents, that the volume can be said to be portable or enchiridial. The materials, indeed, which are contained in the first of the works in our list would, if printed in the usual style, make a good large and rather corpulent octavo; and we suppose that the same may be predicated of the second when it is completed.

It is not worth while finding fault with the form of the publication when the material is good. But it must not be concealed that it is something like a misapplication of terms to apply to treatises like the present the denomination of manual.

The work of Mr Taylor, which alone is completed, may be regarded as a full systematic Treatise on the subject of Medical Jurisprudence. The principal and most obvious defect is, that he does not commence with any general account of the subject or its divisions. This is certainly difficult to do in a subject in which the different divisions are not very naturally or intimately connected; and this therefore, perhaps, is the reason that the author does not attempt it.

Mr Taylor arranges his subject in the following order. First, he treats of Poisons very much in the usual manner, and following the ordinary distinctions and classification in twenty-eight chapters. Then he considers the subject of Wounds and Injuries in fourteen chapters. In the third place, he treats of the subject of Infanticide in eight chapters.

The subjects of Drowning, Hanging, Strangulation, and Suffocation follow; and then come Death by Lightning, Cold, and Starvation.

Next to these are placed the subjects of Rape and Pregnancy, and the questions to which they give rise; then birth, inheritance, and legitimacy; and, last of all, is treated the subject of Insanity, and the manifold questions to which that disease gives rise, either as a reason for confining its subjects, or as affecting their responsibility as social and moral agents.

This volume certainly presents a very excellent view, which may be named both full and condensed, of the present state of knowledge on Medical Jurisprudence. The author has illustrated many of the doubtful points of the science by good and interesting cases. He has, in general, shown much judgment in the examination of the difficult and ambiguous questions. The chemical part of the work is, as might be expected, the most elaborate. But the whole treatise is so ably prepared that we have no hesitation in recommending it as a very useful guide to the student.

Of the work of Dr Guy only two parts have appeared, and it would be wrong to speak of it without seeing the whole, which we expected would have been ere this time. We may justly say, however, that so far as it has proceeded it promises fair to be a useful work on Legal Medicine.

When the third and concluding part appears we expect to give a more particular account of the work.

PART III.

MEDICAL INTELLIGENCE.

ANATOMY AND PHYSIOLOGY.

Researches on the structure of the Liver. By MM. WEBER, KRUCKENBERG, and MULLER. (*Müller's Archives, No.* 4. 1843.)—According to the researches of Kiernan, the ramifications of the *vena portæ* communicate with the ramifications of the hepatic vein,—the latter being placed in the centre, the first in the periphery of the acini or lobules of the liver. The researches of Weber and Kruckenberg were undertaken with the view of demonstrating the existence of vascular capillary reticulations, which are the last ramifications of the *vena portæ* and the radicles of the hepatic veins. The meshes of this net-work are occupied by the biliary canals, which of themselves form a net-work. According to these writers, the hepatic artery communicates directly either with the *vena portæ*, or with the capillary net-work; thus differing from Kiernau, who described this artery as the vessel which furnished the supply of nutritive fluid to the biliary canals, and the envelopes which come from the capsule of Glisson and isolate the lobules.

MM. Weber and Kruckenberg deny the existence of lobules in the liver. According to them, the liver is composed of a continuous mass, which is not divided into distinct lobules by clefts or cellular tissue, but in which the biliary canals and blood-vessels are placed in distinct channels. The smallest ramifications of the hepatic vein are distributed between those of the *vena portæ*, but at a sensible distance from each other, which space is filled up by cellular tissue, and by the biliary canals. The regular form of these small branches causes the aspect of a division into lobules.

M. Müller, on the other hand, sustains the old opinion of the division of the liver into distinct lobules; and if MM. Weber and Kruckenberg have not seen the cellular and fibrous coat which envelopes the lobules, it is, according to him, to be attributed to the injections, which, by distending the vessels, have compressed the interlobular divisions. To exhibit the lobules distinctly, it was only necessary to macerate the liver in acetic acid (vinegar) for eight days. The acetic acid destroyed the cellular tissue, whilst the lobes remained suspended by the ramuscules of the hepatic vein, as Kiernan had formerly shown.

The terminations of the biliary canals have as yet escaped Müller's research. But MM. Weber and Kruckenberg have succeeded in demonstrating the existence of a net-work formed by the biliary canaliculi, in the meshes of which are placed the vascular capillaries.

Functions of the Lacteals and of the Veins. By M. CHATIN. (*Comptes Rendus des Seances de l'Academie des Sciences,* 4th March 1844.)—M.

Chatin's experiments were made for the purpose of ascertaining whether poisonous substances were introduced into the circulation by means of the veins, or by that of the lacteals.

1. Eight dogs were poisoned by introducing into their stomachs a large dose of arsenic, mixed with milk, and then tying the œsophagus. The blood of all these animals, both that extracted from the heart and from the large vessels, was incinerated with nitrate of potash. The product of the incineration, furnished by means of the apparatus of Marsh, a distinct ring, and spots of arsenic, which, on being subjected to the proper tests, furnished all the characters of that metal. The chyle collected from the thoracic duct of these eight dogs, added together, and treated in the same manner as the blood, yielded not the slightest trace of arsenic.

2. Like experiments were made on dogs by substituting tartar-emetic for the arsenic. The animals were killed about an hour after the administration of the poison by opening the carotid arteries. The antimony was found in the blood, but not even a trace in the chyle.

3. A quantity of blood abstracted from different individuals who had been taking antimony in large doses, furnished a notable quantity of that metal.

On the Development of Animalculæ in the Alimentary Canal during Digestion. By MM. GRUBY and DELAFOND. (*Comptes Rendus des Seances de l'Academie des Sciences*, December 11, 1843.)—It appears from the researches of MM. Gruby and Delafond that, in a very great number of animals, the process of digestion is accompanied by the development of infusory animalculæ within the stomach and intestines. Four species were met with in the Ruminantia; seven in the horse; two in the dog; but only one in the hog. These animalculæ exist in such numbers that in the sheep it was reckoned that not less than from 18 to 30 ounces in weight of them were present,—a quantity very nearly equal to a fifth part of the fluid in which they swim. These infusoria are seen alive in prodigious quantity in the first and second stomachs of the Ruminantia; but in the third and fourth stomachs of these animals their carapaces, or crustaceous envelopes, are alone seen. In the horse, however, they are found alive throughout the whole length of the intestinal tube, excepting in the narrowed part of the colon and the rectum. From these facts MM. Gruby and Delafond conclude, that these animalculæ are digested in the fourth stomach (caillette) of the Ruminantia, and in the colon of the horse, and that both of these organs thus furnish a nutritive animal juice. They also infer that a fifth part of the vegetable matters introduced into the stomach of the herbivorous animals is destined to produce infusoria, which, digested in their turn, furnish animal matters to the general nutritive materials, and that this conclusion appears to receive a strong degree of probability from the circumstance, that in carnivorous or omnivorous animals, as the dog and pig, the infusoria are not only much less numerous and smaller in size, but consist of only one or two species.

Case of Formation of Fœtus in the Ovary. By Dr GRISCOM. (*New-York Journal of Medicine*, July 1843.)—In the month of October 1841 Mrs H. consulted Dr Griscom on account of an hæmorrhagic discharge from the vagina, she being pregnant, as she supposed, about seven months. This discharge was described as consisting of very thin blood of a bright colour, but which never coagulated. It had existed for about five months, coming away in greater or less quantity every day, but had been more constant of late. Her general health was good. The abdomen did not present that uniform and peculiar firmness which attends pregnancy at the seventh month. Midway between the umbilicus and pubes the fingers could be pushed back nearly to the promontory of the sacrum, when a tumour of an elongated form, and very tender to the touch, could be detected filling the right iliac fossa. A smaller tumour was also detected on the left side of the median line. She had felt distinct motion till within a month. On the 24th of October pains resembling those of labour came on. Severe pain shortly afterwards was experienced over the whole abdomen, which became very tender to the touch. The pulse rose to 130, the tongue became coated, and the hæmorrhage from the vagina ceased. Notwithstanding active antiphlogistics, the peritonitic symptoms increased, and she died in about a week from their commencement.

The whole peritoneal surface was injected, and a large quantity of reddish purulent matter found effused into the cavity of the abdomen. In the right iliac fossa was a large tumour of a blue colour, and, towards the left, was the uterus, about the size of that organ in the second month of pregnancy. Extensive effusions of recent lymph connected the bowels to the tumour. The tumour occupied the whole pelvic cavity. The uterus was very fleshy, contained a small quantity of mucus, and its inner surface was copiously dotted with red points. The left Fallopian tube and ovary were sound. The right Fallopian tube was sound to its fimbriated extremities, which were attached to the tumour. The tumour was then seen to be formed of the right ovarium. When the tumour was opened, there was found within it "a perfectly formed fœtus, of about six months, placenta and all complete." The child seemed to have been dead some time, was very much softened, and the placenta was partially converted into a purulent matter resembling that found in the peritoneal cavity. Ulcerated openings existed here and there over the surface of the tumour, which communicated with the internal cavity of the tumour, and allowed the matter to escape into the peritoneal sac. The ovarium consisted of a large sac, of a mingled muscular and membranous texture; its walls being about the thickness of a silver dollar. Drs Boyd, Swett, and Smith of New York were present at the autopsy.

Chemical Phenomena of Incubation. By MM. BAUDRIMONT and MARTIN SAINT-ANGE. (*Comptes Rendus des Seances de l'Academie des Sciences*, 26th December 1843.)—From the experiments of MM. Baudrimont and Martin Saint-Ange, it appears that the presence of oxygen is indispensable for the evolution of the embryo of the chick *in ovo*. During this evolution the eggs lose water, and transform the oxygen of the air into carbonic acid. The quantity of carbon burned augments in proportion as incubation advances; but the proportion of water, which is, without doubt, the production of the combustion of hydrogen, remains nearly invariable. This double combustion of carbon and of hydrogen accounts for the temperature proper of eggs; for Hunter proved that fecundated eggs possess the power of generating heat, by finding that they resisted congelation longer than unfecundated eggs. MM. Baudrimont and Martin Saint-Ange have not yet ascertained whether azote undergoes any important chemical transformation during incubation, but they are engaged in experiments with the view of ascertaining this point.

CHEMISTRY.

Additional Note on a proposed New Thermometric Scale. By THOMAS STRATTON, M. D. Edinburgh. (*In a Letter to Sir George Ballingall, M. D.*)—In the Edinburgh Medical and Surgical Journal for January 1844, page 228, there is described a scale for the graduating of thermometers, according to which mercury freezes at zero, snow melts at 72°, and water boils at 252°. An instrument maker, in constructing a thermometer with this scale, would have to observe the melting of snow, and mark 72° at this point, then ascertain the part of the tube which corresponded to the boiling of water, and divide the interval into 180 degrees; from 72° the numbers require to be carried downwards to zero, at which point mercury freezes. This graduation provides for a sufficient range of temperature, without having to go below zero, and would answer for most parts of British North America, and similar climates; almost the only situations where a British observer has to note a degree of natural cold, greater than this, are in the northern parts of the Hudson's Bay territory, and during polar voyages, for which there might be constructed spirit-thermometers, graduated below zero of the new scale.

In a climate like this of Canada, the superiority of the new scale over that of Fahrenheit is that, in the former, zero is not so much in the way, and it is not necessary to be constantly saying and writing "above" and "below zero;" the zero of Celsius, and also of Reaumur, being higher up in the scale of natural temperature, are still more inconvenient. I happened to-day to observe in the London Encyclopædia, (Tegg, London, 1830, vol. 22, page 49), "Fahrenheit began his scale at the point at which snow and salt congeal. Kirwan proposes the freezing point of mercury." I was not before aware that any one had proposed to take this point as a commencement, though, as it has so many advantages, it is likely that it has occurred to many.

It appears to be unavoidable having degrees below zero, as it is impossible to ascertain the extreme limit of cold, and it would be unphilosophical to assume one, and therefore the only remaining resource is to place zero as much out of the way as can conveniently be done.

The recommendation and employment of any new scale by the Royal Societies of London and Edinburgh, the British Association, and other influential bodies and individuals, would induce many to adopt it, while a few years would suffice for the present stock of instruments to be broken. Should the new scale be adopted it would be expedient that it should be introduced by the old one; for some years, both scales being engraved on the brass or ivory, one on each side of the tube.

There would, no doubt, be great difficulty in driving out of use an instrument so long established as Fahrenheit's thermometer, but that the thing is possible is shown by the expulsion and disuse of previous thermometers, not so perfect as that of Fahrenheit.

Penetanguishene, Canada, March 28, 1844.

MEDICAL PATHOLOGY.

Ossification and Obliteration of the Vena Portæ. By Professor GINTRAC. (*Journal de Médecine de Bourdeaux*, Jan. 1844.)—A man, 45 years of age, an old retired soldier, but employed in teaching recruits, two years ago was treated for palpitations and difficult respiration, and abdominal dropsy. He was then only partially relieved; all his symptoms remaining more or less. On the 10th of June he was admitted into the Hospital St André, when he presented the following symptoms:—Dyspnœa, increased on walking; strong tumultuous action of the heart, with distinct *bruit de soufflet* and slight *bruit de raphe* over the sternal region. Pulse calm, but full. Tongue dry, and red on the margins and tip, but covered on the centre with a brown fur. Abdomen distended, tympanitic in the centre, dull, and with distinct fluctuation at the sides. Gums bleeding, but not livid; epistaxis, thirst, anorexia, headach, &c. He died in a few days.

The cellular tissue was generally infiltrated with a serous fluid. The heart was large. The aorta at its origin, and for nearly the half of its extent, had a mottled appearance, consisting of reddish spots, rounded white projecting spots of a cartilaginous consistence, and of others which had a pustular aspect. The peritoneal cavity, otherwise healthy in appearance, contained several pounds of liquid serum. The liver was pale or whitish, and irregularly wrinkled or mammillated on its surface. The gall-bladder contained a medium quantity of somewhat thickish yellow bile. The biliary ducts had a normal disposition. The *vena portæ*, above the junction of the splenic and superior mesenteric viens, was filled completely by an old clot which adhered to the inner membrane. The clot was solid, and of a pretty deep black colour. At the same part of the vein several osseous plates were observed several lines in diameter. They were placed between the inner and middle coats of the vein, but had but little adhesion to either. All the abdominal veins which ended in these vessels were gorged with blood, and were varicose.

Professor Gintrac attributed the ascites to the obliteration and ossification of the *vena portæ*, and considered that this case proved, that though obliteration of that vessel probably modified the secretion of the bile, it did not prevent it altogether, but that it materially interfered with the nutrition of the liver. The blood of the *vena portæ*, he hence infers, contributed to the nutrition of the liver, but is not indispensable to the secretion of bile.

Laceration of the Vena Cava Inferior. By M. Netter. (*Gazette Medicale de Strasbourg*, August 1843.)—A man, 44 years of age, fell, on the 23d of April 1843, from the first floor of a house. He remained some time in a state of collapse; but at length reaction was established, and, under antiphlogistic treatment, he seemed to progress towards convalescence. On the 30th, however, distinct febrile symptoms came on. The respiratory murmurs were distinct, but normal over all the anterior portion of the chest. When the ear was applied to the precordial region, a distinct, but deep-seated, bellows-sound was detected, and was heard at the same time as the sounds of the heart, but was distinct from them. On the posterior part of the chest a very hard bellows-sound was heard, apparently quite superficial, and distinctly synchronous with the arterial pulsations. This sound had a strong resemblance to the placental or uterine sound, from which it only differed in its greater intensity. It extended over the whole posterior and lower part of the chest, and completely masked the respiratory murmur. No unusual dulness over the part was detected. The five following days oppression in breathing, cough, and bloody expectoration were the prominent symptoms. After this the lower extremities became œdematous, the other symptoms becoming worse and worse, and he died ten days after the inflammatory attack. The bellows-sound became more distinct over a larger space as he got worse, and was at last heard from the scapula down the whole back to the sacrum and even the hips.

Effusion of blood was found in different parts of the peritoneum. The gastro-hepatic omentum had a dark-red colour, evidently produced by effused venous blood. Between the kidneys, on the left side of the vertebral column, a bluish fluctuating tumour was detected, having an elongated triangular form. On the right this tumour was covered by the aorta, which passed over its anterior surface, and was thus detached from its usual position. Still more to the right side the tumour was in contact with the posterior face of the *vena cava*, with which it communicated directly above the origin of the primitive iliac veins. Its cavity contained venous blood and solid clots of blood. The common iliac veins, but especially the right, were dilated, and their cavities were filled with consistent clots.

Hemiplegia from tying the common Carotid Artery. By M. Sedillot. (*Gazette Medicale de Paris*, 3d September 1842.)—M. Sedillot applied a ligature to the right common carotid artery, to arrest hæmorrhage from a wound behind the right branch of the lower jaw. Complete hemiplegia of the left side of the body and of the right side of the face followed, and the man lost his intelligence so far as scarcely to comprehend questions put to him. He died nine days after the application of the ligature, and the *post mortem* examination showed that the hemiplegic symptoms had resulted from the right side of the brain having been deprived of its due proportion of arterial blood.

On the Erysipelatous Fever, as it appeared in the northern section of Vermont and New Hampshire, in North America, in the years 1842–3. By Drs Hall and Dexter. (*American Journal of the Medical Sciences*, January 1844.)—The epidemic about to be described was first noticed in the State of Vermont in 1841, but was reported to have been previously observed in Canada. It occurred chiefly along both banks of the River Connecticut, and also extended to many towns situate back from the banks of that river. In its southerly progress along the banks of that river it suddenly stopped and proceeded in a westward direction, leaving a tract of country about 30 miles long and 20 miles wide, perfectly free from the disease. The disease prevailed in its most malignant form from the Canada line, in the course of the Connecticut river, a distance of 100 miles, from this point westwardly to the Lake Champlain, and eastwardly to the State of Maine.

The disease was ushered in by many of the premonitory symptoms of pyrexia. Sore throat, more or less severe, enlarged tonsils and submaxillary glands, difficult deglutition, and sometimes painful respiration, with lassitude, pain in the back, nausea, and retching were among the prominent symptoms. The breath was uncommonly offensive; the tongue covered with a grayish-white slimy fur, beneath which it was deep red; the bowels more or less constipated; the pulse frequent and depressed; the hands and feet cold and clammy; the skin contracted; and the general expression shrunken and haggard. A rigor and subsequent reaction generally followed these symptoms; but the chilliness was commonly complained of during the whole of the hot and sweating stage, even when the body was bathed in the copious acrid perspiration. On the third or fourth day an efflorescence appeared on the skin in the form of erysipelas. This erysipelatous efflorescence usually appeared first on the side of the neck or face, presenting an acutely sensible and circumscribed red spot. When first noticed it might be covered with the point of the finger, but rapidly spread upwards, with a defined line of demarkation on its upper margin till it covered the whole face and scalp. It was most commonly confined to one side of the body, but often two-thirds of that side would be affected at once. The inflamed condition of the mucous membrane of the throat generally abated on the appearance of the external inflammation; but whenever this receded, as it frequently did during the course of the disease, the throat again became affected, and was again relieved on the reappearance of the erysipelatous efflorescence.

The subcutaneous cellular tissue was, in the bad cases, seriously affected. The disorganization which resulted detached all connection between the skin and muscles, and sometimes even the muscles from the bones. Its place was supplied by a semiputrid highly corrosive fluid, in which floated long shreds of cellular membrane reduced to the appearance and consistence of wet rotten linen. The fluid was so corrosive that it destroyed steel instruments used in opening the abscesses or the dead body, as rapidly as if it were nitric acid.

In some cases, instead of the diffuse erysipelatous efflorescence, there appeared carbuncular swellings which discharged a foul offensive ichor, and rapidly went on to gangrene. In the most rapidly fatal cases the fibro-serous membranes were affected, and death sometimes occurred in 24 or 48 hours from the first invasion of the disease.

In all the severe, and in many of the mild cases, erratic pains, resembling the rheumatic, were present, and if with these any fulness of the part were observed, it was found sooner or later to end in the formation of extensive subcutaneous diffuse abscess. In all cases the quantity of purulent matter discharged was very great, the diffuse abscess often extending over the pectoral muscle up to the axilla, and all the course of the *latissimus dorsi* and scapular muscles.

The duration of the disease was very uncertain, depending on the organ affected, and on the extent of disorganization produced by the diffuse subcutaneous cellular inflammation. It is mentioned as a remarkable fact, and one which may throw some light on the nature of puerperal peritonitis, that, during the prevalence of the erysipelatous fever, puerperal peritonitis was unusually frequent, and the persons affected, provided they were within the range of country devastated by the epidemic, mostly died. In the county of Caledonia, State of Vermont, thirty cases of puerperal peritonitis occurred, and only one recovered. In Bath, out of a population of 1500 or 1600, 20 mothers died under puerperal peritonitis, and 40 with the epidemic erysipelas.

The disease was so easily communicated by dissection that few bodies were opened to ascertain the pathological condition of the organs. Three physicians are recorded to have thus fallen victims to this disease. In those cases examined the peritoneal cavity was found more or less injected and dark coloured, and several pints of offensive purulent matter effused within its cavity. In one case the liver and uterus were softened, injected and dark coloured; in another the omentum was covered with ulcerations and shreds of lymph.

The treatment was various; but the most successful, when the disease set in with chills, was to give warm teas, as of sago, balm, &c. with solution of acetate of ammonia, every three hours, and external heat till free perspiration was induced. Counter irritation and Dover's powder were useful when pain was complained of in any particular region. When the patients were subjected to blood-letting and active remedies, the fatality of the disease was greatly increased, excepting in those cases in which the sero-fibrous membranes were affected, when bleeding was the only remedy to be depended on. But the early use of quinine, carbonate of ammonia, camphor, and wine, were in very many cases decidedly beneficial. Lunar caustic, freely applied over the surface, in some cases arrested the erysipelatous efflorescence; in other cases blisters or iodine had the same effect. In many, however, all remedies failed.

Statistics of Pulmonary Consumption in the Cities of Boston, New-York, and Philadelphia for thirty years. By George Hayward, M.D. (*American Journal of the Medical Sciences,* April 1843.)—With the view of ascertaining whether pulmonary consumption was on the increase or decrease in America, Dr Hayward has compared in a tabular form the whole number of deaths with those from consumption, as they respectively occurred in the three cities of Boston, New-York, and Philadelphia. In order to afford better means of comparison, the thirty years are divided into three decades.

BOSTON.

	Total Mortality.				
1st 10 years,	8,741.	Of this No. died by consumption,	1896,	or 1 in	4.622
2d	12,379.	- - -	2046,	—	6.050
3d	17,406.	- - -	2396,	—	7.587
For 30 years,	38,616		6243,	—	6.185

NEW-YORK.

	Total Mortality.				
1st 10 years,	27,080.	Of this No. died by consumption,	6,061,	or 1 in	4.451
2d	45,547.	- - -	8,010,	—	5.686
3d	79,853.	- - -	13,415,	—	5.952
For 30 years,	152,480		27,486	—	5.547

PHILADELPHIA.

	Total Mortality.				
1st 10 years,	23,582.	Of this No. died by consumption,	3,629,	or 1 in	6.498
2d	37,114.	- - -	5,522,	—	6.721
3d	52,900.	- - -	7,070,	—	7.482
For 30 years,	113,596		16,221	—	7.003

It is remarked that the most striking fact brought to light by these tables is the great decrease of deaths by consumption in these cities—a decrease great in all, but relatively greater in Boston than in the others. The tables show that Philadelphia has suffered less from consumption than the other cities, the average number of deaths by that disease being only 1 in every 7.003 deaths, while in Boston it was 1 in 6.185, and in New York, 1 in 5.547.

The bills of mortality of the city of Boston show that there has been a striking and progressive improvement as to pulmonary consumption since the year 1811. By the United States census of 1810, Boston contained 33,250 inhabitants; in 1820, 43,298; in 1830, 61,392; and in 1840, 93,452. In 1811, when the population had probably not increased at all from the preceding year, as it was a period of great commercial depression, the whole number of deaths was 742, of which 221 were from consumption; while in 1840, with a population nearly three times as great, and with nearly three times as many deaths, there were only 19 more cases of consumption, or 240 from that disease—not quite 1 in 8 deaths, or scarcely 3 in 1000 inhabitants.

Dr Hayward endeavours to account for this decreased mortality by attributing it in no small degree to the great improvements which have taken place in living during the last thirty years, and to the increased comforts of life enjoyed by all classes of the community. They are better fed, better clothed, live in more comfortable houses, indulge in less excesses, and pay more attention to personal cleanliness. The introduction of cotton fabrics, and their general use, is especially referred to as a means which "has no doubt contributed essentially to this desirable result."

Strangulated Intestine from Rotation of the Sigmoid Flexure of the Colon. By Dr BIGELOW of Boston. (*American Journal of the Medical Sciences*, October 1843.)—The Attorney-General of the United States was seized, on the 16th June 1841, with severe symptoms resembling colic. Laxative enemata and counter-irritants failed to relieve the constipation or the pain. Next day the pain and tenderness of abdomen had increased, but injections could not be pushed beyond the sigmoid flexure of the colon, and drastic purgatives failed to act. Elastic tubes were passed by the rectum, and water injected through them, but also without effect. A hot bath and large doses of opium gave some relief and a few hours' sleep, but on the next day tympanitic distension of the abdomen came on, and the pulse rose to 140. There was no nausea or vomiting. Two tobacco injections and inflation of the rectum by means of bellows still failed to give relief. The tobacco injections contained each half-a-drachm of tobacco, and were each retained for half an hour, but caused no symptoms of narcotism or prostration. He sunk early next morning.

The abdomen was found greatly distended, and nearly filled by the sigmoid flexure of the colon, which extended across the abdomen into the right hypochondrium, and was so distended that its external circumference was fifteen inches. It was of a dusky green colour, as if from incipient gangrene, but there was no softening of its texture, nor diminution of its external polish. The two extremities of the flexure, that connected with the transverse colon above, and that with the rectum below, were twisted together about the mesentery as an axis, into a firm cord or neck about an inch in diameter. On being carefully untwisted, the whole included portion was found to have made four turns, or two entire revolutions on itself. There was no line of demarcation between the healthy and strangulated portions, nor was there externally any remains of old disease on that part. The small intestines and the colon were somewhat distended, but the rectum was contracted. The cavity of the peritoneum contained a small quantity of turbid reddish fluid, and on one of the convolutions of the small intestines, there was a small effusion of lymph.

On an epidemic pseudo-syphilitic Affection. By M. Alies. (*Journal de Medecine de Lyon*, September 1843.)—The village of R., near Luxeuil, consists of about 700 souls. In 1840 and 1841, symptoms, which bore a strong resemblance to syphilis, were observed in 80 of the inhabitants. The principal and most constant phenomena consisted of mucous tubercles, developed at the margin of the anus, and of the genital organs. Public rumour attributed this affection to an individual who had contracted the venereal disease when from home, and at his return had communicated it to his family, from which centre it had spread over most of the village. M. Alies hesitates to pronounce this affection true syphilis, though its symptoms, its seat, and the fact of its having been easily removed by the use of mercurials, as well as its obstinately resisting other treatment in those who at first refused to submit to that treatment, favour the belief of its syphilitic origin. But, on the other hand, its apparently epidemic transmission, seeing it occurred in very many individuals who could not be traced to have had any connection with the diseased, and, above all, the fact of no secondary symptoms being ever observed, throw great doubts as to its being truly syphilitic.

Rare form of Small-Pox. By Dr Wotherspoon. (*New-York Journal of Medicine*, March 1844.) Dr Wotherspoon describes three cases of a rare form of small-pox which casually occurred during the last three years, in the New York City Hospital. He considers that the disease which he observed has been only observed in three other instances, viz. a case recorded by Dr Pond, one in the Medico-Chirurgical Review, and one in Ryan's Journal. The disease, however, is evidently the same as that described by Sydenham as the black small-pox, and by Morton, Huxham, and De Haen, as a very fatal form, which they variously term the bloody, gangrenous, septic, or measly small-pox. In the three cases described by Dr Wotherspoon, the disease set in with chills, and violent pains in the back, followed by complete prostration of strength, fever, loss of appetite, intense thirst, nausea, and symptoms of gastric irritation. After a period, varying from thirty-six to forty-eight hours, a deep purplish rush appeared over the neck, limbs, and trunk, and the mucous membrane seemed to be similarly affected. Numerous vesicles, filled with clear or bloody serum, appeared over the whole surface, interspersed with points of various dark hues, evidently produced by extravasation of blood. After the appearance of the eruption they became comparatively comfortable, owing to a cessation of the intense pain of the back.

On the fourth or fifth days they became rapidly worse in spite of all treatment. The deep purple colour of the surface continued. The extravasations of blood were in large patches, and more numerous. The conjunctiva, from extravasation of blood, assumed a deep red colour, and formed a sanguineous chemosis around the cornea. An intolerable sickening, cadaverous fetor was given off, and pervaded the apartment. Hemorrhages took place from the skin, mouth, and nostrils, and every discharge was coloured with blood. The extremities at length became cold, the pulse more and more frequent and weak, and muttering delirium was shortly followed by death.

On dissection, every organ was found with its vessels congested with blood, and with bloody fluid effused in numerous situations. The muscular and parenchymatous tissues were, in addition, much softened; and the blood itself was changed into a dark cherry-red watery fluid, containing merely a few thin, friable, black coagula.

MEDICAL THERAPEUTICS.

Succinate of Ammonia the best remedy in Delirium Tremens. By M. SCHARN. (*Journal de Pharmacie,* March 1844.)—From meeting with cases of delirium tremens which resisted the usual remedies, and from considering that disease to consist of nothing else than drunkenness in its extreme state, M. Scharn thought it would be removable by the same means. As ammonia is very efficacious in the removing the symptoms depending on the one state, he thought they would be equally valuable in the other. He accordingly tried the succinate of ammonia, and by the administration of this simple remedy, he has seen the most furious delirium quelled as by enchantment, and the whole disease removed in a few hours, without the aid of any other remedy.

Successful employment of Arsenic in a Case of Peritoneal Dropsy. By DR DEBAVAY. (*Gazette Medicale de Paris,* 9th March 1844.)—A woman, 38 years of age, and the mother of three children, applied to Dr Debavay on account of abdominal dropsy which had persisted for fifteen months. After her last confinement she had been attacked with puerperal peritonitis, from which she imperfectly recovered. Shortly after this the abdomen began to enlarge, for which she was subjected to various kinds of treatment, from which she only experienced temporary relief. The abdomen was tense, hard, and with a distinct sense of fluctuation. None of the abdominal organs, however, appeared to be affected. Her general health and appetite were good. From the known beneficial effects of arsenic in other diseases, Dr Debavay resolved to try its action in this case. He commenced by administering one pill morning and evening, containing the twentieth part of a grain of arsenic. For the first few days no sensible effects followed this dose; but soon slight colic pains and diarrhœa followed, and so increased, when three pills were given daily, that the medicine was obliged to be suspended. When again resumed, only one or two pills were taken in the course of the day. At the end of six weeks the abdomen was less tense, and notably diminished in size, at the same time the urine was more limpid and increased in quantity. The skin, too, which was before dry, became soft and moist. The arsenic was continued at intervals for six months, by which time all symptoms of dropsy had ceased; the catamenia, which had been suppressed, had returned, and she enjoyed perfect health.

SURGERY.

Extirpation of a Tumour of the Parotid. By DR HOSACK of New-York. (*New-York Journal of Medicine,* March 1844.)—A coloured woman, 38 years of age, when vomiting about six years ago, felt something give way behind the angle of the jaw. On applying the hand to the spot she perceived a small hard tumour, which was painful on being pressed. It gradually increased till it attained the size of a hen's egg, when it became stationary, and continued so till about a year ago, when it again began to grow rapidly. The tumour was situated directly under the ear behind the angle of the lower jaw, and appeared to be about the size of a large potato, and had a nodulated surface. Indistinct fluctuation was perceived within it, but when firm pressure was made a resisting hard substance was felt, in which a small aperture could be felt, and through which the fluid part of the tumour could be pressed. The tumour did not project into the mouth or fauces, nor interfere much with the motions of the jaw. As its nature seemed obscure, a small puncture was made into its most prominent part, where the fluctuation was most distinct, and from this flowed about a table spoonful of a clear amber-coloured fluid; a quantity of a pure black fluid also came away, but did not mix with the clear fluid. As the solid part of the tumour appeared to be of a scirrhous nature, and the black fluid evacuated bore a considerable resemblance to melanotic matter, it was resolved to extirpate the mass.

An incision was commenced behind the conch of the ear, and carried over the surface of the tumour to the sound structure of the neck. A second incision was begun directly below the lobe of the ear, and carried over the tumour so as to join the first at its termination. The triangular flap of integument was then raised. The exposed part of the tumour had a true melanotic look. The parotid gland was healthy on that part which lay over the face; but the rest seemed to form part of the tumour, so as to leave no doubt that it formed the seat of the disease. The external carotid artery was then sought for, and a ligature passed around it where it entered the tumour, and also where it left it. The mass was then dissected out, tying each artery as it was cut. The previous evacuation of the contents of the tumour greatly facilitated the operation. The parotidean vein required a ligature, and before the operation was completed fifteen vessels were tied. The *portio dura* of the seventh pair of nerves was also divided, as well as many of its branches. The carotid artery was not divided till the long posterior conical projection of the parotid gland had been completely separated by dissection from all its other attachments. About 25 ounces of blood were lost during the operation. On inspecting the wound after the operation not a vestige of the gland could be seen excepting a small portion over the cheek, which it was not considered advisable to remove as it was perfectly sound. The wound was brought together by the ordinary suture, and supported by adhesive straps. No unfavourable symptom occurred to interrupt recovery.

The tumour presented, in some respects, the characters of an encysted tumour, but the walls were resistant and dense, and, in the posterior part, of great thickness, and presented the true scirrhous character. This portion had adhered to the carotid artery for a considerable extent. The interior of the sac was very irregular, and every concavity seemed to be lined with a pseudo-membranous formation, of a very black colour. This black matter appeared to be very slightly connected with the sac, as it was readily removable with the handle of the knife, and by slight maceration.

Removal of inverted Uterus by the Ligature. By Dr ESSELMAN. (*American Journal of the Medical Sciences*, January 1844.)—A lady, 32 years of age, had been in bad health ever since the birth of her only child twelve years previously. She was at that time attended by an old woman, had suffered severely from flooding, and never perfectly recovered. She was constantly annoyed by bearing-down pains, pain and weakness in the back, the pain stretching down the thighs. Every now and then she suffered from severe uterine hæmorrhage, and at other times was subject to *fluor albus*. For these complaints she was subjected to various medical treatment, some considering her disease to be prolapsus of the uterus, others polypus. In the belief it was polypus, from finding a rounded tumour apparently projecting from the *os uteri*, and after consultations with several practitioners, Dr Esselman applied a ligature to the projecting tumour, having two hours previously administered a large draught of camphor, laudanum, and hartshorn. The tightening of the ligature excited such intense pain that the draught required to be repeated. For the first five hours thereafter her strength was quite prostrated, and her pulse barely perceptible. Reaction then slowly set in, she became more composed, and passed a tolerable night. The ligature was tightened every morning for eighteen days, at which time it came away, when, instead of proving a polypus, the tumour was found to consist of the uterus itself. The cure proved tedious, as the vagina was ulcerated, and poured out a copious sanious discharge. She recovered, however, under the use of generous diet, wine, and quinine, taken internally, and injections of chloride of lime and nitrate of silver to the vagina. For twelve months after her recovery she required frequent blood-lettings and active purgation to relieve headach, vertigo, and general plethora, occasioned, it was conceived, by the premature suspension of the catamenial secretion.

Case of Vaginal Hysterotomy. By Dr Bedford. (*New-York Journal of Medicine*, March 1844.)—Dr Bedford was called in consultation with several other practitioners to a lady who had been in violent labour for 24 hours, which had not, however, caused the slightest progress in the delivery. Her sufferings were of the most intense nature, so much so, that her cries had attracted a crowd of persons around the door. She had previously had two children, and her labours were described to have been easy. On examination, a solid resisting tumour, evidently the child's head, was felt at the upper strait of the pelvis, through the walls of the strongly contracting uterus, but no *os uteri* could be detected. In carrying the finger upwards and backwards towards the *cul de sac* of the vagina two fleshy bridles were discovered, extending from that portion of the vagina to a point of the uterus, where some roughness was perceived, but no aperture. Between the bridles and this rough point distinct cicatrices were felt, of which this rough surface was one. On closely questioning her as to whether she had ever sustained any injury, she confessed that, on five previous occasions when she felt herself pregnant, she had applied to a quack, who gave her powders which produced miscarriage, but that on this occasion the powders had failed. She again applied to the quack, who proposed to "probe" her; but as a sum was demanded for this operation above what she could afford to pay, she attempted to probe herself. She used a whalebone probe several times, which produced considerable pain, followed by a discharge of blood. It was thus apparent that the present state of matters had been produced by violence inflicted on the mouth and neck of the uterus. It was therefore resolved to incise the womb in the most depending part. A probe-pointed bistoury, covered with linen to within four lines of its point, was therefore carried into the vagina along the finger, introduced into the uterus, where the roughness was detected, and carried first to the right and then to the left. As it was however feared that this aperture would not be sufficient, an incision was also carried through the posterior lip. The uterus contracting violently, rapidly dilated the aperture thus made, and in ten minutes a living child was born. The patient recovered rapidly, without one untoward symptom.

Case of Transplantation of the Cornea in Man. By Dr Kissam. (*New-York Journal of Medicine*, March 1844.) An Irishman, 35 years of age, with one eye completely destroyed, and the other suffering from staphyloma, with adhering iris, was the subject of this case As transplantation of the cornea appeared the only mode of relief which afforded any chance of recovery of vision, that operation was resolved on. The cornea to be transplanted was obtained from a pig six months old; but before it could be cut from the animal, it was found necessary to extract the whole eye from its socket. The central portion of the cornea, nearly half as large as the thumb nail, was cut out, placed on the end of a cork, and two ligatures passed through its margins at opposite sides.

With Beer's cataract knife, the necessary portion of the protruding opaque cornea of the patient was removed, the pig's cornea laid over the aperture in the eye, and secured in its position by the two ligatures which were on a line with the angle of the tarsi. The ligatures were cut short and the eyelids closed. On separating the eyelids twenty-four hours afterwards, violent chemosis was found to have occurred; but in twelve hours more this had so far subsided as to allow the state of the eye to be ascertained. As the transplanted cornea was found adherent, the ligatures were removed. The chemosis was so great that the raised conjunctiva served as a cushion to protect the transplanted cornea from the irritation of the moving tarsi; but, more important still, it overlapped the transplanted cornea, and thus secured it in its place. This perhaps contributed to secure the adhesion, as, instead of cut edge being united to cut edge, the transplanted cornea was applied over the margins of the patient's cornea, so that its cut surface was lying over the conjunctival surface.

The vision was improved immediately after the operation, but as the humours themselves were found diseased, it was still imperfect. The cornea continued transparent for a fortnight, when it became opaque; and in the course of a month it was absorbed. The operation was performed in the autumn of 1838, in the presence of Drs Paul, Roberts, J. B. Kissam, and Pratt.

Further experiments on the Transplantation of the Cornea. By M. FELDMANN. (*Comptes Rendus des Seances de l'Academie des Sciences,* 4th March 1844.)—In this, his second memoir on transplantation of the transparent cornea from one animal to another, M. Feldmann details the results of twenty experiments made in the laboratory of M. Flourens, at the Jardin des Plantes. The conclusions at which he arrives, relative to this operation, are the following:—

1. The union of the transplanted cornea, with that of the eye operated on, takes place either by the first or second intention, or partly by both. When the margins of the cornea are in contact, or only separated by a very trifling space, union takes place by the first intention. In this last case a distinct plastic exudation, furnished by the neighbouring parts, but especially by the iris, unites the margins of the cornea. Union by the second intention takes place when the margins of the cornea are separated a considerable distance from each other.

2. The vascular connection, when the cornea unites by the first intention, takes place in the following manner:—One or more large vessels proceeding from the conjunctiva advance to the edge of the transparent cornea. The blood from these vessels is then seen to penetrate the new cornea, either by forming new vessels through it, or by filling the old ones, till then invisible. It always happens that at first these vessels are excessively fine, and have a radiated or arborescent distribution relative to the main trunk.

3. The transparency of the transplanted cornea, which subsists for a few days after the operation, disappears afterwards. The cornea changes its colour in proportion as the vascular apparatus is more fully developed within it; it ends by becoming more or less opake.

4. The aqueous humour, lost during the operation, is quickly reproduced, so as in some cases to swell out the transplanted cornea to a globular form shortly after the operation.

5. When a part of the cornea floats, as it were, in the purulent matters, secreted by the neighbouring parts, and the eyelids are allowed to rub over its detached margin, it becomes softened, and soon loses its conjunctival covering. The cornea never in this case adheres.

6. The cornea, after it has been fairly transplanted, ends by becoming shrivelled and useless, in consequence of the absorption of a half or more of its extent. It sometimes becomes flattened, sometimes globular. The points of cicatrization adhere to the iris.

On Conical Cornea. By James H. Pickford, M. D., M. A. I. A., &c. (*Dublin Journal of Medical Science*, January 1841.)—In this essay Dr Pickford undertakes to give a new explanation of the nature of an imperfectly understood disease of the eye, and an improved and rectified method of treatment.

The conical and glistening cornea was first well described by Leveillé in 1802, by Mr James Wardrop in 1808, and shortly after by Mr Robert Lyall, in the seventh volume of this Journal. It is known to consist in a peculiar prominent appearance of one or both corneæ, which are observed to taper to an obtuse point, so that the cornea no longer forms a segment of a sphere as in the natural state, but a sort of short flat cone, with a round apex. At the same time the cornea has a peculiar glistening, shining or luminous aspect, totally different from the appearance of the healthy cornea, and which is easily recognized by a little observation. The point of it indeed appears too clear, and looks like a drop of water, or a bit of transparent glass; while it conveys the idea that the individual has no distinct vision, which indeed he has not.

Sir David Brewster found by inspection with powerful glasses, that the surface of the cornea invariably presented numerous small spherical elevations and depressions. It was also found by Mr Middlemore, in one case in which he examined the cornea after death, that its laminæ were less movable upon each other than natural, its circumference was of a natural and ordinary degree of thickness, but its apex was much thinner than usual, and rendered opake in its exterior only; for its neural surface, even at the apex, was perfectly transparent. In other respects it did not appear to have undergone any change. Jaeger, in like manner, found the apex of the cornea attenuated by a central depression, with marginal thickening, to near its union with the sclerotic.

All the modes of explaining this change hitherto suggested Dr Pickford rejects; and he infers from various considerations, that conical cornea depends on faulty action, induced by debility of the nerves of the cornea, of its absorbent vessels, calling for an increased deposit from the nutrient capillaries to repair the mischief arising from such faulty action; and he regards the disease as analogous to hypertrophy, with dilatation of the ventricles of the heart and to aneurism. In short, maintaining that the cornea is well provided with blood-vessels, nerves, and absorbents, he says that there is first disease or disordered action in the nerves, then, as a consequence of this, the absorbents act on the apex of the cornea, and eat it so much, that it is attenuated, and meanwhile the vessels deposit outside towards the margin of the cornea. Hence he infers that conical cornea depends upon some disturbance in the functions of the great sympathetic spinal nerves and *par vagum;* producing, through the medium of the lenticular ganglion and fifth pair of nerves, faulty action of the nutrient capillaries and absorbent vessels of the cornea itself.

Such is the pathology, the theory of the disease. Next comes the treatment. As the disease consists essentially in debility and elongation of the capillaries of the part, with morbid deposits; so the cure lies in their retraction by means of tonics. This is to be accomplished by beginning at the source of the evil, viz. the sympathetic and splanchnic nerves. Emetics and purgatives, by the powerful influence they induce on the gastric, associate and consensual nerves, restore the healthy functions of the weakened nutrient and absorbent vessels, the result of which is a slow but progressive retraction of the diseased corneal growth, and a consequent restoration of vision.

With these indications in view Dr Pickford prescribes occasional emetics of tartrate of antimony or sulphate of zinc, cathartics, and iodine. The great benefit, however, he ascribes to the use of emetics given every morning for one week, two weeks, or three weeks. At one time he ordered one grain of tartar emetic and four drachms of sulphate of magnesia every morning. Now he tells us he is in the habit of prescribing sulphate of zinc, the good effects of which he finds equal to those of tartar emetic, while its operation is much less distressing to the patient. In illustration of these principles he gives three cases.

OBSTETRICS.

Case of child surviving though born the sixth month. By Dr SHIPMAN. (*American Journal of the Medical Sciences,* April 1843.)—Mrs Eldridge, aged 24, and mother of two children, when about the sixth month of pregnancy, received a fall which brought on premature labour. The infant was barely alive, with little motion, and too feeble to cry. It had no nails on its hands or feet, nor hair on the scalp, and the cranium was imperfectly ossified. The child was wrapped in a blanket, and laid away, as it was not expected to live. On examining it, however, after the mother was attended to, it was found exhibiting some symptoms of life, and was washed, dressed, and fed. It was unable to take the breast, was seized with convulsions about a week after birth, which recurred at intervals for three weeks. At the end of seven weeks it was weighed for the first time, and found to weigh only 1 pound 10 ounces. At the time the account was written, it was ten months old, was lively, playful, and healthy, and weighed 10 pounds 8 ounces. Dr Shipman regrets he did not at the time take notes of the state of the eyes, its length, &c., which he was only prevented doing from expecting it every day to die.

TOXICOLOGY.

Case of Poisoning by Verdigris. By M. GUILLO. (*Journal des Connaissances Medico-Chirurgicales,* December 1843.)—A woman, 28 years of age, in consequence of a quarrel with her husband, swallowed a large dose of verdigris. Very shortly after she was seized with great anxiety, vomiting, acute pains, and swelling of the abdomen, sensation of burning heat in the throat, coldness, and severe cramp in the extremities, oppressed pulse, swelling of the face, and sparkling eyes. When seen by a surgeon she had an emetic administered, which brought away a quantity of pultaceous matter, composed of half-digested food, but without traces of the poison. This was on the 20th of October.

On the 21st, at two o'clock of the morning, the deglutition was very painful; there was hardness and swelling of the neck; the abdomen was tympanitic, and painful on the least pressure; the countenance was heavy; the face flushed, and the pulse oppressed. About two pounds of a distinctly greenish fluid were evacuated, mixed with a few streaks of blood. During the day deglutition became more and more difficult. The mucous membrane of the mouth became altered in appearance in several places, and the stiffness and swelling of the neck notably increased. The face and eyelids were swollen and red, and the eyes prominent; the abdomen became flattened, but extremely sensible, and the rectum so painful that the patient refused to allow clysters to be given her.

On the 22d a tendency to coma was observed. The face was pale; the lips swollen; the gums ulcerated; and there was a copious discharge of viscid saliva. The neck had become softer, and many ecchymosed spots were observed on it. A copious stool was passed, the first since the poisoning, and acetate of copper was detected in it in pretty large quantity. Some blood, which was drawn, presented the inflammatory coat. She had several spasmodic fits after taking a draught of infusion of marsh mallows.

On the 23d a quantity of viscid glairy matters were vomited. They were of a greenish colour, and tinged with blood. She suffered also occasionally from spasmodic attacks.

On the 24th epistaxis and general cramps came on, and the evacuations by stool and by urine were completely suppressed. In the afternoon the whole body became cold, and convulsions supervened, which lasted till the evening, but abated on the occurrence of free evacuations by stool and urine.

On the 25th she was somewhat better, and able to pronounce a few words; and the next day the ulcers of the mouth began to heal. On the 27th she vomited some greenish glairy matters, but from this date gradually amended, though complaining occasionally of epigastric uneasiness.

Effects of a large dose of Arsenic taken by a Lunatic. By Dr SHIPMAN. (*American Journal of the Medical Sciences*, October 1843.)—In the year 1838, a gentleman, who had been insane for several years, took a tea-spoonful of the white oxide of arsenic, with the view of destroying himself. It was taken immediately after a hearty dinner, and washed down with a draught of water. Soon after, he vomited freely, and diarrhœa came on and continued for several days. He had no unusual burning pain in the stomach or bowels, nor, excepting the diarrhœa, any symptoms of having taken arsenic. His appetite was even but little affected. Within a week he was seized with severe pain in the legs and arms, which he compared to the gnawing of rats, or the boring of a gimblet into the bones. There was some swelling of the legs and feet, but no preternatural heat. This was followed by a partial paralysis of the limbs, which remained obstinate for months, notwithstanding various remedies were tried for its removal. Strychnine was at last given, and continued for several weeks, when the paralysis gradually left the limbs, and his health was restored. With his returning health returned his soundness of mind, so that he has been since enabled to resume his business as a merchant, which he conducts judiciously and with success. He has had no return of the insanity up to this date.

Poisoning by Œnanthe Crocata, or Hemlock Dropwort. By Mr P. BOSSEY, Woolwich. (*Medical Gazette*, Vol. xxxiv., p. 288.)—Though cases of poisoning by the dropwort have been recorded by so many authors, that it seems unreasonable to doubt the energy of this plant as a toxicological agent, yet cases are also recorded, and experiments have been made with the plant, which would lead to the inference, that, at all times and in all places, its energy is by no means uniform. Dr Christison read before the Royal Society this season, a series of observations and experiments, which showed that the plant was often eaten in Scotland at least, and even in some parts of England, without any noxious effect, or almost any effect whatever; and even from various trials made with the plant as it is found in the marshes at Woolwich, by Mr Pereira and himself, in the summer of 1842, it could scarcely be inferred that the dropwort was a very active poison. It is not, perhaps, easy to explain these discordant results. Dr Christison seems, in his observations, to ascribe them to varieties in soil, in climate, and perhaps in the temperature of the season. But whether we are destined ever to understand the obscure influence of these agents, it is important at least to collect cases, and, above all, cases which demonstrate unequivocally the agency and properties of the plant. An instructive series of cases has recently been furnished by a melancholy accident occurring among the convicts at Woolwich.

On the 4th February 1843, twenty-one convicts were employed at the mortar-mill, situate on the banks of a canal in the Royal Arsenal at Woolwich. At 11 o'clock A. M., eight or ten of them went round the building to an adjacent pond of water to wash their spades and boots. One man (Chamberlaine) strayed away from the rest and found this plant (*Œnanthe crocata*) growing near the bank of the canal, the leaves and roots of which he mistook for celery. He dug up some, washed, tasted, and conveyed it to his companions. Several of the men returned to the spot, obtained more of the roots, ate them freely, and distributed portions among their fellow workmen who remained within the building.

At twenty minutes past eleven, under the direction of the keeper, they were all about to fall into ranks, for the purpose of returning on board-ship to dinner, (most of them still eating and putting roots into their pockets), when one Wilkinson, without apparent warning, fell down in violent convulsions. The struggling was soon over. He became better, but had a wild expression in the countenance, which was pale; and in a short time he had another fit. Whilst they were laying this man on a shutter, a second individual fell (Knight); and before they reached the yard adjoining the hulk, a third (Wilson), and a fourth (Salt), had also fallen and were convulsed.

Mr Bossey arrived about a quarter before twelve, at which time nine stout young men were convulsed and insensible. The three worst, Wilkinson, Knight, and Wilson, were lying in a shed; Chamberlaine, Gundle, and Jeffs had just fallen in the yard; and Williams, Jones, and Salt were struggling on the deck of the vessel.

1. It was manifest that Wilkinson was dying. The bloated livid face, the bloody froth about the mouth and nostrils, the stertorous and convulsive breathing, and the extreme prostration and insensibility, indicated that every remedial measure was useless. Nothing was done but to raise the head and shoulders; and in a few minutes he died.

2. Knight had been violently and repeatedly convulsed, and appeared fast hastening into the same apoplectic condition. He was insensible, speechless, with the pupils dilated, the face swollen and livid, the breathing laboured, and the limbs convulsed. Deglutition being impracticable, and the jaws fixed, they were forced asunder, and water, by means of the stomach pump, was copiously introduced into and withdrawn from the stomach. With the fluid some leaves were extracted; but the instrument was worked with the greatest difficulty, owing to the violence of the convulsions. Death took place in a quarter of an hour.

3. Wilson had assisted to carry the two former. When near the yard he looked pale, and fell convulsed very violently. After the fit he was restless; consciousness partly returned; he answered yes when his name was loudly called; and swallowed an emetic solution of sulphate of copper. No vomiting followed. The convulsions were renewed; the stomach pump was introduced, but withdrew only fluids. Collapse threatening immediate dissolution followed. The face was pale, the pupils dilated, breathing convulsive, and death appeared at hand. The stomach pump was again used, and small portions of the root and leaves were withdrawn. Convulsions returned violently, and about half-past twelve death took place.

Emetics of salt and mustard, with warm water, were administered to those who had fallen in the yard, under which they vomited freely, and discharged a large quantity of imperfectly masticated root, and were thereby greatly relieved. The convulsions ceased, sensation and reason were restored, but there remained giddiness, paleness of the face, dilated pupils, coldness of the extremities, much weakness, severe rigors, and a slow feeble pulse. Further vomiting was promoted, and more of the root discharged. Friction and warmth were applied to the extremities, while ammonia and rum with thin gruel and other drinks were administered internally till reaction was more fully established.

Emetic doses of the sulphates of zinc and copper, and also mustard and water, were given without effect to the patients lying on the deck of the vessel. They were also bled very largely both from the arms and jugular veins. The introduction into, and removal of warm water from, the stomach by the pump brought away small portions of the noxious roots. Cold affusion upon the head perseveringly used lessened the struggling and produced some exhaustion. In three cases (Salt, Williams, and Burgess) the subsequent fits became less violent; they passed into a state of maniacal delirium, with much jactitation of the limbs, and after some hours were removed into the hospital.

4. But in one more patient (Jones) all these remedies were ineffectual: he died convulsed at a quarter before 1 o'clock. As a last effort, the trachea was carefully opened by an incision, and artificial respiration kept up, but life was quite extinct.

Several of the men who had eaten the root, seeing the others suffer, took the salt water emetic with success, and had no symptoms of being poisoned; others felt giddiness and faintness in a slight degree, and at 6 P.M. there were upon examination eleven who required watching, and were therefore sent into hospital."

5. Jer. Chamberlane, aged 24, continued ill till the 9th, and was discharged cured on the 17th of February.

6. John Williams, aged 22, continued to suffer till the 10th, and was discharged recovered also on the 17th.

7. William Jeffs, aged 23, continued ill till past the 13th, then on the 22d he had symptoms of abdominal inflammation, but was discharged cured on the 3d of March.

8. Joseph Salt, aged 17, continued very ill with delirium, hurried breathing, eventually cough and profuse expectoration, and great abdominal tenderness, and died at 3 P. M. on the 13th February, nine days after eating the plant.

9. Abel Burgess continued ill with convulsions, then laborious breathing, bloody expectoration, and afterwards delirium and great debility, and died exhausted about 2 P. M. on the 15th of February, eleven days after eating the plant.

The remaining patients did not suffer any urgent symptoms Under the free use of purgatives they passed considerable quantities of the root, and were in a few days discharged.

The deaths were therefore six; viz. Wilkinson, Peter Knight, Wilson, Jones, Joseph Salt, and Abel Burgess. The appearances on dissection are given in the whole.

1. *Wilkinson.*—The appearance of the body very stout and muscular; extremely rigid; the fingers stubbornly contracted; the thumbs turned in upon the palm; the nails bluish. General lividity, not very intense, but in patches anteriorly, while the whole dorsal surface presented a deep purple hue, except in the hams, where the hue was redder, and on the parts subjected to pressure, as the scapulæ and nates, over which it was absent. The scrotum and penis slightly livid; the former not distended.

The countenance livid, somewhat swollen, especially beneath the jaw; the eyelids somewhat open; conjunctivæ slightly congested above and laterally; slight opacity of the cornea; the pupils dilated, the irides being about a line in breadth; the lips, gums, and ears purple; a frothy secretion filled the nostrils; the tongue bitten at the tip, with an appearance of blood between the teeth.

The chest flattened, contracted above, duller than usual on percussion; there was unusual distension and resonance of the abdomen, especially in the epigastric region.

On dividing the scalp, fluid blood escaped more freely than usual; the pericranium natural; the skull very thick; on the interior of the skull were a few injected veins: the dura-matral surface was thought to be not much injected; the longitudinal sinus contained only fluid blood, not the slightest trace of a coagulum. The veins of the *pia mater* had a natural appearance; they certainly were not distended, as they did not reach a level with the top of the convolutions. Not more than the usual quantity of blood in the sinuses, but it was of a very black colour and perfectly fluid. There was a slight degree of serous effusion, a clear gelatinous deposit under the arachnoid, and some globules of air were seen in the veins. The processes of *pia mater* dipping between the convolutions were darker than usual; the *plexus choroides* also darker, being of a plum or damson colour. The cortical structure of the cerebrum was not redder than is usual in recent brains; more bloody points were seen in the *centrum ovale*; no serum in the ventricles; some veins were ramifying on the *corpora striata*; the substance of the corpora and thalami were thought to be slightly redder than usual. The veins and sinuses beneath the posterior lobes of the cerebrum were fuller; the veins on the *pons varolii*, the *medulla oblongata*, and about the roots of the nerves, turgid. The substance of the cerebellum, pons, and medulla, was somewhat redder than usual. On dividing the veins and arteries of the spinal marrow, a quantity of fluid blood flowed into the base of the skull. No water at the base.

The veins beneath the integuments of the back were congested. On opening the vertebral canal the *theca spinalis* had a reddened appearance; the vertebral vessels were injected; the veins and sinuses filled with black fluid blood; the substance of the spinal marrow, especially its internal structure, redder than usual; at the lowest portion, in the lumbar region, there was a little serum.

The tongue was large, and covered with a white fur; the papillæ at the base remarkably elevated and vascular; the soft palate, tonsils, and pharynx, covered with a glairy mucus, and presenting a general blush of dull and somewhat livid vascularity, deepest in those parts where the fluid gravitated, as in the little cavities between the epiglottis and the tongue. The epiglottis dull, with vascular lividity, turgid veins being seen upon it. The pharynx and œsophagus had a white appearance, contained some mucus, and portions of the root.

The trachea and bronchi were lessened in diameter, appearing contracted, and the lining membrane intensely injected with dark blood. This injection was partly concealed by a thick smearing of reddish mucus, which also filled up the smaller bronchial tubes. The lungs were purple and heavy, being gorged with fluid blood; and there were several recent small extravasations of blood scattered through their substance.

The stomach and intestines externally were pink in colour and distended with air; the stomach also contained a little fluid, some of the root, and was, with the intestines, every where lined with a thick viscid mucus. The mucous membrane was thrown into folds, forming prominent ridges, intersecting each other like meshes. After washing away the mucus, when the membrane was placed under water its surface had a mammillated appearance, the follicles being particularly distinct, erect, and abundant. Held up before the light, the pink colour of the tunics deepened by the action of the atmosphere upon the dark blood in the vessels; there were also minute points of extravasation, and dark lines in the course of the venous trunks.

There was a great deal of mucus, and minute portions of the root in the small intestines, especially in the lower part of the ileum.

The state of the heart was natural, but the blood found in it, and also in the great vessels, was very black and fluid.

No trace of decomposition had occurred.

2. The second body examined was that of Peter Knight (No. 2), which presented similar external appearances, and was thought to be equally livid, but had somewhat less muscular rigidity.

A greater quantity of blood than is usual flowed from the incisions made in opening the head and spine. The principal abnormal circumstances noted on this inspection were the veins of the *pia mater* very much distended, causing extensive arborescence both on the surface and between the convolutions of the cerebrum. The *plexus choroides* had a dark damson colour, the *velum interpositum* much injected, and many vessels ramifying upon the *corpora striata*. The veins of the cerebellum, at the base of the brain, and the basilary veins, were also in an equal degree distended. The tentorium was extremely blue in colour, not from extravasation, but from great injection.

The substance of the brain in all its parts was more vascular, and exhibited more bloody points in the sections, as did also the *pons varolii*, *medulla oblongata*, and spinal marrow. The roots of the nerves were also red from injection; the sinuses of the spinal marrow gorged. About two drachms of serum in the ventricles; four drachms were collected at the base; and there was also general, but not great, effusion under the arachnoid.

A slight contusion on the soft palate had resulted from the passage of the stomach tube, but the whole of the throat, the root of the tongue, the glottis, and epiglottis, were much reddened; the follicles at the base of the tongue were particularly large and prominent. The œsophagus reddened through its full extent; at the lower end the cuticular lining with its fimbriated edge particularly evident.

The glottis and *sacculus laryngis* were lined with frothy mucus; the trachea most intensely injected of a deep purple almost black colour. The bronchi were similar in colour, and plugged by mucus. The lungs were very dark, extremely congested, and in many places apoplectic effusions had occurred.

The peritoneal coat of the stomach had a pink colour, especially intense about the cardiac orifice. Large black veins were running along its lesser curvature, and branching down its sides. The mucous membrane thickened thrown into large rugæ; follicles prominent with vascular apices, and of a pinkish colour; pylorus contracted; there was much extremely viscid mucus strongly adherent. The state of the intestines similar to that described in the last case.

3. The body of Wilson (3) was also examined, and in the head and spine the same appearances were noted.

In the mouth and throat, the papillæ of the tongue remarkably enlarged. In the fauces, pharynx, and larynx, a remarkable vascular lividity, most intense about the epiglottis and soft palate. The *sacculus laryngis* and trachea contained frothy mucus; the lining membrane of the trachea also highly congested, thickened, and presenting the same characteristic, though rather less distinct, appearances than the others.

There was an accidental effusion of blood behind the œsophagus, and between its tunics at the lower part. The stomach contained a pint of turbid fluid, with some portion of the masticated root. The stomach and intestines both exhibited the same pink colour externally. The bronchi were likewise congested. The lungs were extremely livid; fluid blood and serum freely flowed from every incision. In some parts the congestion was as great as in cases of pulmonary apoplexy. The follicles in the œsophagus and stomach very distinct. There was a large quantity of viscid mucus in the stomach, especially near the pylorus.

4. The only difference noted on the inspection of the body of Jones was the presence of a layer of extravasated fluid blood beneath the *pia mater* covering both hemispheres of the brain. The colour of the membranes was quite blue. The effects upon the other organs were quite analogous to those already related in the other cases.

5. Joseph Salt.—The trachea and bronchi were injected, and the smaller bronchi filled with mucus. The left pleura was lined with lymph, and its cavity filled with serous effusion. The stomach and intestines were pink

on their external surface, the intestines glued together by adhesive matter; and there was much peritoneal effusion, with flakes of adhesive lymph. The mucous membrane of the stomach and bowels was softened, thickened, and everywhere coated with an abundance of mucus. On washing this off, the membrane was much injected. The vessels of the brain were more injected than usual, and there was slight serous effusion beneath the arachnoid.

6. Abel Burgess.—On examination of the body there was found a considerable development of the papillæ at the root of the tongue, and of the mucous follicles thoroughout the œsophagus, stomach, and intestines. The surface of the mucous membrane was lined with a tenacious secretion, which, being washed away, presented the membrane itself, thickened, softened, and more than usually vascular.

The lining membrane of the trachea and bronchi was deeply injected, the smaller tubes being choked with a frothy bloody mucus. The lower portion of the right lung in the state of red hepatization. Both lungs were heavy, contained much blood; and interspersed throughout their substance, but especially in the right, were various spots of ecchymosis from extravasated blood,—probably effused during the primary convulsions.

The right pleura was adherent, and lined with the products of recent inflammation; and in the back of its cavity there was considerable serous effusion lying upon a surface covered with fibrinous lymph of a pale straw colour.

The pericardium was largely distended with a greenish coloured serum.

The vessels of the *pia mater* were fuller than usual. The brain was not unhealthy; it presented rather more bloody points, and there was also slight serous effusion.

The most important conclusions flowing from the facts disclosed by these dissections are the following. The first effect of the poison seems to have been on the cerebro-spinal axis; and even afterwards it appears to have operated violently on this part of the economy.

The next most remarkable effect is the great irritation and inflammation of all the mucous surfaces; first, those of the gastro-enteric organs, which was natural, and, secondly, in a degree not much less in the bronchial division. Associated with the former, was the morbid development of the follicular apparatus.

A third effect appears to have been inflammation of several of the serous membranes, the pleura, and peritoneum.

The lungs also appear to have been hepatized.

Lastly, the blood was softened, or unusually fluid.

MEDICAL POLICE.

Remarkable Trial for Murder, in which death was not owing to wounds received. (*American Journal of the Medical Sciences*, April 1843.)—Charlotte Ewing was indicted at Mobile, state of Alabama, in November 1842, for the murder of her husband. They were players by profession, and on the night of 25th March, Andrew Ewing and his wife came off the stage together, and, while doing so, high words passed between them. He then struck his wife and went down stairs, she following. In a few minutes thereafter she was seen standing at the foot of the stair, holding a dagger in her hand, and exclaiming she had killed him. He was found lying across the threshold of the dressing-room, speechless, and with two wounds in his arm, and shortly thereafter expired.

On the trial it was proved he had enjoyed good health for several years, and played parts which required great physical strength; also, that he played at many games requiring a great amount of bodily exertion, but never complained of fatigue or difficulty of respiration. For the prosecution, Dr Kelly deponed, that he was sent for, and found Ewing dead; that there were two wounds upon the right arm, by which the basilar artery was cut in two places. These wounds were near the olecranon, on the inner and lower side of the arm. There was another wound upon the left side of the body, between the false ribs and the iliac region, which ran obliquely upwards. Dr Kelly did not attend the dissection, but he found that the dagger had penetrated at least two or three inches in the direction of the stomach and spleen. He could not say whether the deceased was killed by the wounds he received. He did not observe any arterial blood issuing from the wound.

Dr Levert examined the body, and at first supposed that the wounds were the cause of death. The abdomen was full and distended. Two slight wounds were on the right forearm, and one in the left hypochondriac region. There was no hæmorrhage from this wound, but on moving its lips with the finger and thumb, a small quantity of dark-coloured blood issued from it. On opening the abdomen, its whole cavity was found filled with blood, which had caused the distension above noticed. Dr Levert's first impression was, that some important vessel had been cut across by the dagger; but on tracing the course of the wound, it was ascertained that no vessel of any consequence had been injured. The instrument had entered the left hypochondriac region, just under the margin of the false ribs; its direction was a little upwards and inwards; it passed through the mesocolon, near to the gut, but without wounding it, and passed into the cavity of the stomach, near its larger extremity, but without passing through its opposite wall. The blood was therefore carefully sponged out, in order to discover whence the enormous quantity of blood had come, as it could not have come from the wounds. When the blood was removed, a large aneurismal tumour was found almost entirely filling the right iliac fossa. This aneurism had given way a little below and to the right of the duodenum. It was found on the descending aorta, and, from the large quantity of fibrinous matter which it contained, and the attenuated condition of its parietes, it was evidently of very long standing. As the dagger had not passed near to the aneurism, and its rupture was the cause of death, and the high state of excitement of the deceased was believed sufficient to induce its rupture at that time.

The jury returned a verdict of not guilty.

We received several months ago a letter addressed to Sir Robert Peel by the Professor of Military Surgery in this University, recommending the establishment of lectureships in his department in other schools of medicine. In addition to the statement laid before the Minister, Sir George Ballingall has, in a letter to the Editor of the Medico-Chirurgical Review, annexed to our present Number, entered into some explanatory details of his views, more particularly addressed to his professional brethren.

To the subject of Military Medicine and Military Surgery, comprehending especially Military Hygiene, we have more than once adverted in this Journal; and we may repeat what we have already stated, that it is of the utmost importance that those destined for the public service shall be well-informed in their profession, and instructed in the nature of the duties which they may be called on to perform, when they receive an appointment in the medical department of the army, in whatever way that information may be obtained. The requisite information may be obtained in various modes, partly from books, no doubt, and partly from the lectures of experienced officers who have themselves served in the army.

ON

SCHOOLS OF NAVAL & MILITARY SURGERY.

BY SIR GEORGE BALLINGALL, M.D.

To the Editor of the Medico-Chirurgical Review.

Edinburgh, 10th April, 1844.

SIR,

Some six months ago I addressed a Letter to Sir Robert Peel, urging the institution of a Professorship of Military Medicine and Surgery in London, and another in Dublin. Thinking that I had thus fully discharged a duty which I owed to the public service, it was not my intention to have entered upon the subject again. But the unfavourable notice of that proposal which has appeared in the last Number of your Journal gives me the opportunity, and suggests the propriety, of offering, to my professional brethren, some explanatory details, which would have been altogether out of place in an address to the Minister.

The first erroneous opinion, which pervades, more or less, the whole review of my letter, is that " my principal object was to contrast the liberal provision made by some Foreign States, particularly Prussia and Austria, for the education of their Army Surgeons with the very scanty provision made for the same purpose by our own Government." This contrast is indeed sufficiently striking, but a little reflection would have shewn that it was only mentioned incidentally, with the view of rousing the attention of Government to the subject; and that my principal object was to point out the difference of duties which devolve on our Military and Naval Surgeons, compared with those of the Continental States. It is the special nature of the duties devolving upon medical officers in our service which constitutes the most important part of the contrast, and which requires a " course of instruction," not " different from," but over and above, " that necessary for the Surgeon destined for practice in civil life." Perhaps nothing will render this contrast more intelligible than the different duties which are expected of a Professor of Military Surgery on the Continent and in this country—there I find numerous gentlemen denominated " Professors of Military Surgery "—one, it may be, teaching the practice of Physic, another perhaps operative Surgery, and a third Ophthalmology; but not one of them, so far as I know, giving a course in the least degree a-kin to mine. If no special instruction be necessary to prepare our Army and Navy Surgeons for their peculiar duties; then are the educational establishments at the general hospitals superfluous. If such special instruction be requisite; then will it be most advantageously given where there are most to profit by it—in the Metropolitan Schools—which are frequented both by the old and the young. To which those already in the service naturally will, and to which those preparing for the service necessarily must resort.

The topics which should be embraced in a course of Military Surgery have of late, been frequently specified, and are pretty fully stated by the reviewer in a quotation from Mr. Marshall—we have here, on the one hand, a number of important topics enumerated as fit to be included in a course of Military Hygiene, and we have, on the other hand, an extended *curriculum,* in no one course of which are these topics embraced. Surely out of twenty months allotted to the practice of Physic, and twenty months to the practice of Surgery—in all forty months, or nearly three years and a half—some six months might be spared to those educating for the Army and Navy, to learn something of the new and peculiar duties which, at their very outset, the emergencies of the service may call upon them to execute. I was not eight-and-forty hours in the Army when I was left in charge of two Battalions, and these receiving recruits every hour.

That the Authorities admit the advantage of such a course of instruction is obvious from their permitting Candidates to take six months Military Surgery in place of six months of the Surgical Attendance enjoined on them. By the regulations of the Army, indeed, the former is preferred, a circumstance which the reviewer has failed to notice. The want of similar sources of instruction elsewhere has long been assigned as a principal reason for not making the course of Military Surgery in this University imperative. Did such a course exist in other Schools I think Government would do well to render attendance on it compulsory, and I do not concur in the opinion that such attendance "might justly be considered a hardship." It is however more for the time than the means that I contend. A prevalent and, I believe, a growing opinion exists, that many of the *curricula*, or courses of education, prescribed by the different licensing bodies, are overloaded with compulsory study. Let there be less *compulsory* and more *optional* study, and I have no fear for the result as regards the Professors of Military Surgery. I have some experience of the popularity of this subject, and although, of late years, for reasons very easily explained, the number of students has been less, I have had, in one year, as many as eighty-one paying pupils, and this without the course being *compulsory* on any one of them.

The study of this department would not be thrown away even upon those "who might not be successful in obtaining appointments in the Army or Navy;"—it would not be thrown away either upon themselves or the public. Let gentlemen who may have so qualified themselves have a preference, in the intermediate approval of recruits, and in those numerous cases where troops are dispersed in small parties over the country, apart from their own Surgeons. The Soldiers would then meet with prompt and efficient treatment, while the public would have always at command a body of men who, without prejudice to their qualifications as general Practitioners, would be conversant with the duties of Military Surgeons, and thus competent to act on every emergency.

In a series of questions, more or less relevant, the reviewer asks, "Is the discipline young officers undergo at Fort Pitt or Haslar, and in a Regiment or on Shipboard, not adapted to initiate them into the mysteries of their respective appointments?" Were I to answer this question, as I shall answer others, in the words of some of the most experienced and intelligent Surgeons in the Army, it would not, I apprehend, suit the reviewer's taste, nor serve his purpose; but I prefer, in this instance, speaking in my own person, and will take the liberty of answering one question, *more Scottico*, by asking another. The promptitude, tact, and efficiency, which an attentive young man ultimately acquires under an intelligent and judicious Surgeon no man can more highly appreciate than I do; but might he not be so prepared, by a course of previous instruction, as to acquire all this more speedily, more surely, and more advantageously to the public service? The instructions given at Chatham and at Haslar I am not disposed to undervalue, but I am not singular in the opinion that much of this instruction might be given with equal, if not with more, advantage elsewhere. The opinion that the Museum attached to the General Hospital at Fort Pitt might be placed more advantageously in the Metropolis, is proved by the fact, that a large subscription, at the head of which is the Director-General, has been entered into by the Medical Officers of the Army, "towards the erection of a building for the Museum and Library in London."

With regard to the general Hospitals, as sources of Clinical instruction, I have already hinted at some of the disadvantages under which they labour, but the subject is by no means exhausted. No general Hospital, in this country, can ever be so advantageously employed as the "Hôpitaux d'Instruction" in France, and for this very obvious reason, that we have not here the same diseases to treat as those with which our Military and Naval Surgeons have to contend in our numerous Colonial possessions. So far as Clinical instruction can be combined with the systematic courses of lectures, which I wish to see established in the Metropolitan Schools, it would perhaps best be given by occasional visits to the several Regimental Hospitals of a large Garrison, like London or Dublin; visits such as I have occasionally made, every Session, to the Hospital in Edinburgh Castle. There the pupils would see the diseases which actually prevail amongst the soldiery in this country—not the diseases of invalids. They would see also the interior œconomy and administration of those Regimental infirmaries which have so long been a source of legitimate pride to the Medical Department of the British Army, and to which no parallel is to be found in the other armies of modern or of ancient times. Few of the subjects embraced in the course of Military Surgery are susceptible of clinical illustration. Many of them are highly susceptible of explanation by means of preparations, casts, models, plans, and drawings, such as are provided for the use of the Students of Military Surgery in this University, and of which I have herewith the pleasure to enclose a printed catalogue. Of the subjects enumerated, at pages 8 and 12 of my Letter to Sir Robert Peel, it so happens that the treatment of Ophthalmia, and of the Venereal Disease, are almost the only subjects admitting of any extensive illustration in the Military Hospitals of Great Britain. The illustration of Tropical Diseases in these Hospitals is obviously impossible; and the soil of England—thanks to our gallant Army and Navy!—has never, in our day at least, afforded a field for clinical instruction in the treatment of Wounds.

General Hospitals, however, are necessary for the purposes of the service. One for the whole Army in Great Britain is certainly not a superfluity, and two for the Navy are perhaps still less so. In the Royal Navy the only establishment akin to the Regimental Hospital is the Sick Berth. This upon most occasions is a very limited field, and to the gentlemen of this Department, when returning from foreign service, or attached to ships lying in port, the great Naval Hospitals, with the Collections and Libraries belonging to them, must prove an interesting and valuable source of professional improvement. But the Gentlemen connected with this branch of the service would not, I think, undervalue the importance of a Metropolitan School, in addition to those means of instruction which are necessarily confined to the great sea-ports. It is only justice to these Gentlemen to say that, amidst all my mixed audience, there are none who have been more punctual in their attendance than some of the Naval Surgeons; there are none from whom I have received more interesting communications; and there are none who have shewn more gratitude for the information I was able to give them in return.

This leads me to say a word upon the attendance of Gentlemen already holding Commissions in the service. I am said to have overlooked the fact, that medical officers, even after many years' absence from their native land, rarely obtain sufficient leave to enable them to study. I had, on the contrary, noticed, to the credit of the present Heads of the Medical Departments of the Army and Navy, that they have granted, for this purpose, every facility which the good of the service would possibly permit. I was myself one of fourteen Army Surgeons who graduated here in one year; I have had in one Session as many as thirty-six medical officers entered to the class of Military Surgery; and in all, during the twenty years I have lectured, four hundred and ninety-eight. This no doubt includes all those gentlemen who have been successively stationed here with their Regiments, several who were upon half-pay, and many who have only attended occasionally, or for very short periods; but, with all these deductions, a very considerable number on leave of absence, many of them in an indifferent state of health, have occupied themselves in a praiseworthy manner in attendance upon this, and other classes in the University during the whole Session. A numerous body of Medical Officers, those in the service of the Honourable East India Company, have always, from the prolonged leave of absence which they obtain, ample opportunities of attending lectures, and of this they have very fully availed themselves.

I am held to be unfortunate in alluding to the Statistical Reports on the health of the Army and Navy, as sources of information to be turned to account in the Lecture-room. The conclusions deducible from these reports are indeed "remarkably clear, plain, and unambiguous," and it certainly is not as "the groundwork of a few passing remarks," that the results of these statistical inquiries should be brought before his pupils; but as the most appropriate and most forcible illustrations which a Professor can possibly give, of the observations which it is his duty to make on the geographical distribution of diseases; on medical topography; on the choice of sites for Camps, Cantonments, Barracks, and Hospitals; on the diseases prevalent amongst Europeans on Foreign Stations, and in the vast native Army of India; as well as amongst the various races which constitute our several Colonial Corps, Negroes, Hottentots, Malays, and Cingalese. This, in addition to all other measures for extending their circulation, would be one of the most powerful means of impressing on the rising generation of Army and Navy Surgeons the value of these reports; and I still hold to my opinion that, without this, no justice will be done to them. The measure which I advocate, the establishment of additional Professorships of Military Surgery, has no more strenuous supporter than Major Tulloch, the very author of these reports, and I have reason to believe that this gentleman concurs in every syllable of the following observations by the Editor of the "Naval and Military Gazette."

"No Medical Candidate should be permitted to enter the Army or Navy, who has not attended at least one course of Lectures on Military Surgery, embracing all those circumstances by which the health of Soldiers and Sailors is likely to be affected in different climates; and to enable him to do so, it is indispensible that two medical officers of acknowledged talent in that department of their profession, should be nominated to Chairs in the Universities of London and Dublin, and whose duty it would be, to give to their pupils the benefit of the experience they have acquired in a long course of service, as well as of the vast mass of information annually transmitted, from all foreign stations, to the Medical Departments of the Army and Navy, and which, we feel confident, would most cheerfully be laid open to their inspection for this purpose."

In a subsequent number of the same Gazette, (448,) will be found a communication from several experienced Staff and Regimental Surgeons, who attended the class of Military Surgery in this University during the Session 1840-41. "We rejoice to observe, in a recent number of the Naval and Military Gazette, (January 30th, 1841,) that this subject is taken up in connexion with the valuable statistical inquiries which have lately been made into the sickness and mortality of the Army and Navy, and we are confident that nothing could be more conducive to the public interests than the appointment suggested by the writer, of Professors of Military Surgery in London and Dublin, to whose province it would fall to keep the results of these statistical inquiries constantly before the rising generation of Army and Navy Surgeons."

Amongst the Queries to which I may be expected to reply is the following; "Are the few who have attended the class of Military Surgery, more efficient, more studious, better informed, more accurate observers, or more zealous and industrious in collecting and diffusing the results of their experience and observations, than their brother officers?" This is a somewhat comprehensive question, and it would be somewhat unreasonable to expect me to answer for some seven hundred young men, who have now passed through my hands as pupils, or even for the limited number of these who may have obtained Commissions in the public service. Much of their efficiency must depend upon the "number of hours, which," in the words of the reviewer, "are spent by Medical Officers in the Billiard-room, or dissipated in some of those ingenious methods devised, both in the Army and Navy, to kill time." That, in the opinion of very competent judges, young men so instructed, are well qualified to become efficient medical officers, may be gathered from the following answer to similar questions propounded to two very intelligent and experienced Army Surgeons, Deputy-Inspector Marshall, and the late Staff-surgeon Badenach. "We have a very high opinion of the utility of a course of lectures of this kind. A student who has attended a course of lectures on Military Hygiene, and on Military Medicine, is prepared to avail himself, with great benefit, of the practical advantages of experience, by which means he will be much sooner able to assume in an efficient manner the medical charge of a body of men, than if he had not received such instructions. A course of lectures on Military Surgery is extremely useful, inasmuch as it supplies that information which is peculiarly required by medical officers, and which can be but imperfectly communicated in a course of lectures, either on the practice of Medicine or Surgery."

Again, it is asked, "What practical advantages have resulted, during the last thirty years, from the establishment of a Chair of Military Surgery in Edinburgh?" This is a question very unfit for me to answer, inasmuch as I can scarcely be considered an impartial judge; but, fortunately, I am under no necessity of doing so, the question has been already asked, and already answered, by more competent authority. "Do you think the institution of a class of Military Surgery at Edinburgh, was a beneficial addition to the course of Medical study? In so far as the public service is concerned, the Army, the Navy, and the Service of the Honorable East India Company, I think it has been a most beneficial addition to the Chairs of the Medical School at Edinburgh. Besides Wounds, and what is termed Military Surgery, the Economy of Hospitals, and of Hospital Arrangements, is in this school taught by a gentleman who has served much in the field and in various climates. At the end of the last war upwards of 300 Medical Officers of the Army were placed on half-pay, and it is within my knowledge that many of them profited greatly by attendance upon this class, before they returned to employment on full-pay." Such is the question submitted to, and such is the answer given by Sir James M'Grigor, to the Commissioners for visiting the Scottish Universities in 1827, when I had been about five years in possession of the Chair. With regard to the utility of this class, in subsequent years, I refer the reviewer to a short notice of my Letter to Sir Robert Peel, in the January number of the British and Foreign Medical Review.

Upon one subject, the reviewer's sentiments and mine cordially agree; the propriety of "stimulating the emulative industry of medical officers, and exciting them to professional improvement." Sir Gilbert Blane's Gold Medal as a prize to the Naval Surgeons, is noticed with much approbation in my "Outlines of Military Surgery;" and the successful labours of Mr. Malcolmson, by which he obtained the Prize for the best Essay on "Berriberri and Burning in the Feet," have been quoted in my Class-room from year to year, ever since that Essay was presented to me by the Madras Government. Concurring in all that is proposed upon this point, I am prepared to go even a little further than my reviewer. It is quite possible that a man may write a valuable essay on a given subject, without being an efficient Army Surgeon, and Sir Robert Peel, without taking an additional shilling from the public purse, might offer a boon to industrious students, and to deserving medical officers, ten times more valuable than all the money that can ever be allotted for Prize Essays. For a series of years I have held annually a competition amongst the students of Military Surgery, by way of examination, at the close of the Session, and have been fortunate enough to procure Commissions in the Army or Navy, for several gentlemen who have distinguished themselves in these competitions. No prize can be more valuable, to a candidate for employment in Her Majesty's Service, than a Commission, and none can be more valuable to an officer already in it, than a step of promotion, an appointment to a crack regiment, or to a favourite station. Let this be followed out upon a large scale, let an appointment be given to the most deserving student, and a step of rank to the most deserving officer who may be eligible to it,—their merits, in either case, to be decided by a Committee of experienced officers, as is done at Sandhurst and at Addiscombe. Let this be done annually in each of the three proposed schools, and I can conceive no plan more likely to elevate the character of the Department, or to furnish the service with a well-informed and efficient body of Staff Officers, capable of giving to the Governor of a Colony, or the Commander of an Expedition, the most judicious advice in all that regards the health of his troops.

I have hitherto, Sir, as you will observe, addressed myself generally to the objections which have been brought in opposition to my views, I would now address myself a little more particularly to you and to the reviewer. I have not presumed to think that these objections embody the sentiments of an experienced Naval Surgeon, more especially of one who has acquired experience on the Indian Station, "who has seen the diversified maladies produced by climate, season, constitution, and co-existing circumstances." I am, indeed, hardly left to conjecture on this subject, for we are told, in a subsequent article, that this reviewer is an accomplished Army Surgeon, of which I make no doubt; but I must be permitted to say, that he has, in my opinion, taken a very limited view of his subject,—a view very far short of those lofty conceptions which dictated the Memoir of Mr. John Bell—a memoir which you have seen proper to reprint, and which, although inapplicable at the present day, as regards the general education of our Army and Navy Surgeons, is *more than ever* applicable to that special instruction which it so forcibly inculcates, and which our enlarged establishments, our extended commerce, and our multiplied possessions demand.

Your reviewer cannot have considered all the important bearings of this question, as regards the *collection* as well as the *diffusion* of information on the health of the troops. The pupils of this class have distinguished themselves in all the recent struggles in India. In the first luckless campaign of Affghanistan, no fewer than three of them perished; one of these was Dr. Cardew, whose fate is so feelingly deplored by Lieutenant Eyre, and who had become so great a favourite with the soldiers that, when unable to travel farther, they endeavoured to save him by conveying his almost lifeless body on the carriage of a gun. To this gentleman I was indebted for a communication on fractures when a pupil of the class, and had his life been spared, I might have been in possession of information which is now for ever lost to the world.

I have at this moment lying before me Statistical tables of the sickness and casualties amongst the Queen's troops in China, and returns of Surgical Cases after Sir Charles Napier's recent actions in Scinde, framed for my special use by the Superintending Surgeon of the force, an old pupil of the class. The communications indeed which I have received from my pupils, in the shape of contributions to the Museum, of Returns and reports from distant stations, or of written Essays on subjects discussed in the progress of the Course, are more than I shall ever live to do justice to, or sufficiently to express my gratitude for. Amongst several of these communications which have appeared in print, I may be allowed to notice an Essay on the Comparative Merits of Flap and Circular Amputations by Mr. M'Hardy, and the Essay on the Feigned Diseases of Soldiers and Seamen, by Dr. Gavin. To these I trust I may soon be enabled to add a valuable history of the Medical Department of the British Army, by Dr. Irving.

I am said to have failed in making out a case for additional Professorships of Military Surgery. Upon this point, however, I trust there is room for a difference of opinion; I am told by a distinguished General Officer, who has fought and bled in the service of his country, that I have made out "a most triumphant case," and we are told by a spirited and intelligent professional writer, "that the Government which reads the arguments of Mr. Guthrie and myself, and does nothing, is as clearly condemned as the man who will persist in denying mathematical axioms."

Will your reviewer now allow me to tell him where I think he has failed? He has failed to see how much the institution of such courses of instruction as I recommend is calculated to raise the character of his department. He has failed to see what influence it is calculated to give to medical opinion in the eyes of those holding superior command; and he has failed to see what a waste of life and of health it may obviate, by a perfect fore-knowledge of what has happened, and what may again happen, in any given circumstances.

After having been so freely questioned by the reviewer I shall not I trust be accused of anything rude or uncivil, in asking whether this gentleman has served any considerable portion of his time in the East or West Indies, if so, I have only to observe that he differs very widely in opinion from others similarly situated. Hear the opinion of one of the survivors of the mortality at St. Domingo,—one who subsequently served through the Peninsular war with the elevated rank of a Deputy Inspector of Hospitals, one who, a second time, served in the West Indies, as Chief of the Medical Staff in the Windward and Leeward Islands, and who now lives respected and esteemed by every man who knows him. "I ought," says this voice from St. Domingo, "to have thanked you before now for the little brochure you have sent me, in every word of which I thoroughly agree, and only wish that your arguments may make a suitable impression upon the authorities who have power to give them effect. Your quotation from Mr. John Bell is singularly apposite and good. The most experienced campaigner could not have written better." Hear again the opinion of another gentleman who retired as President of the Medical Board after seven and thirty years' experience in various parts of India;—under all circumstances and situations of intertropical service; in charge of large general hospitals at fixed stations; in field hospitals on actual service; in regimental hospitals moving over various countries, and through different climates; amongst Europeans as well as Natives; and among men, women and children; "I am very much obliged to you for your kind letter and enclosures, I sincerely hope you may be successful, and I think it very probable you will, as the importance of what you recommend must be obvious to every man who thinks at all, and particularly those who have any knowledge of soldiering, or the important duties devolving upon medical men on Foreign service. You have already done so much to benefit and improve the studies of Military Medical Officers that nothing is left but to follow up your plans; which, if adopted in London and Dublin, by the nomination of competent Professors, there cannot be a doubt that the public will be great gainers."

Both the gentlemen above alluded to have written much, and have written well, on the diseases of the troops, and on military hygiene, and their cordial approbation of my views would seem to shew, that they concur in opinion with another talented and experienced Army Surgeon, who, with reference to the very article on which I am now commenting, observed that "the reviewer might as well propose to supplant the Commissariat of an Army by Dr. Kitchener's work on Cookery, as to supply the purposes of so great a School by a book." But it were endless to multiply such quotations, suffice it to say that I am now in possession of six-and-forty letters to the same effect—all these letters, be it observed, wholly unsolicited. One from the Director-General; seven from

Inspectors of Military Hospitals, or of Naval Hospitals and Fleets, eighteen from gentlemen holding the rank of Staff, Regimental, or Naval Surgeons, and the others from Professors in English Universities, from private practitioners, or from General and Field Officers in the Army—the last a class of men whose evidence I am inclined to rate very highly in a question of this kind. A medical man does not, I believe, consider it necessary to acknowledge every pamphlet which may be laid upon his table, and when so many gentlemen of talent and experience have stepped forward to volunteer their opinion in support of my proposal, I may well sit at ease under the strictures of my reviewer.

I conclude by observing that it is from respect to the established reputation of your Journal, from respect to your own character, personal and professional, and from respect to your position as a Naval Surgeon, that I have been mainly induced to enter into this detail. Believe me, Sir, that in what I have now written I am much more solicitous about the promotion of a great public object, than about my personal vindication, or my private interest. I have now been employed for nearly forty years in the study, in the practice, and in the teaching of Military Surgery. I have submitted to the Government, from an imperious sense of duty, my sentiments on this important question, *valeant quantum valere debent*. Gratified by the approbation of my seniors and cotemporaries, both Military and Medical; strong in the rectitude of my purpose; and feeling, personally, but little interested in the result, I shall not again be easily drawn from that quietude which best befits my habits, my present state of health, and my time of life.

I remain, Sir,

Your very faithful and obedient Servant,

GEO. BALLINGALL.

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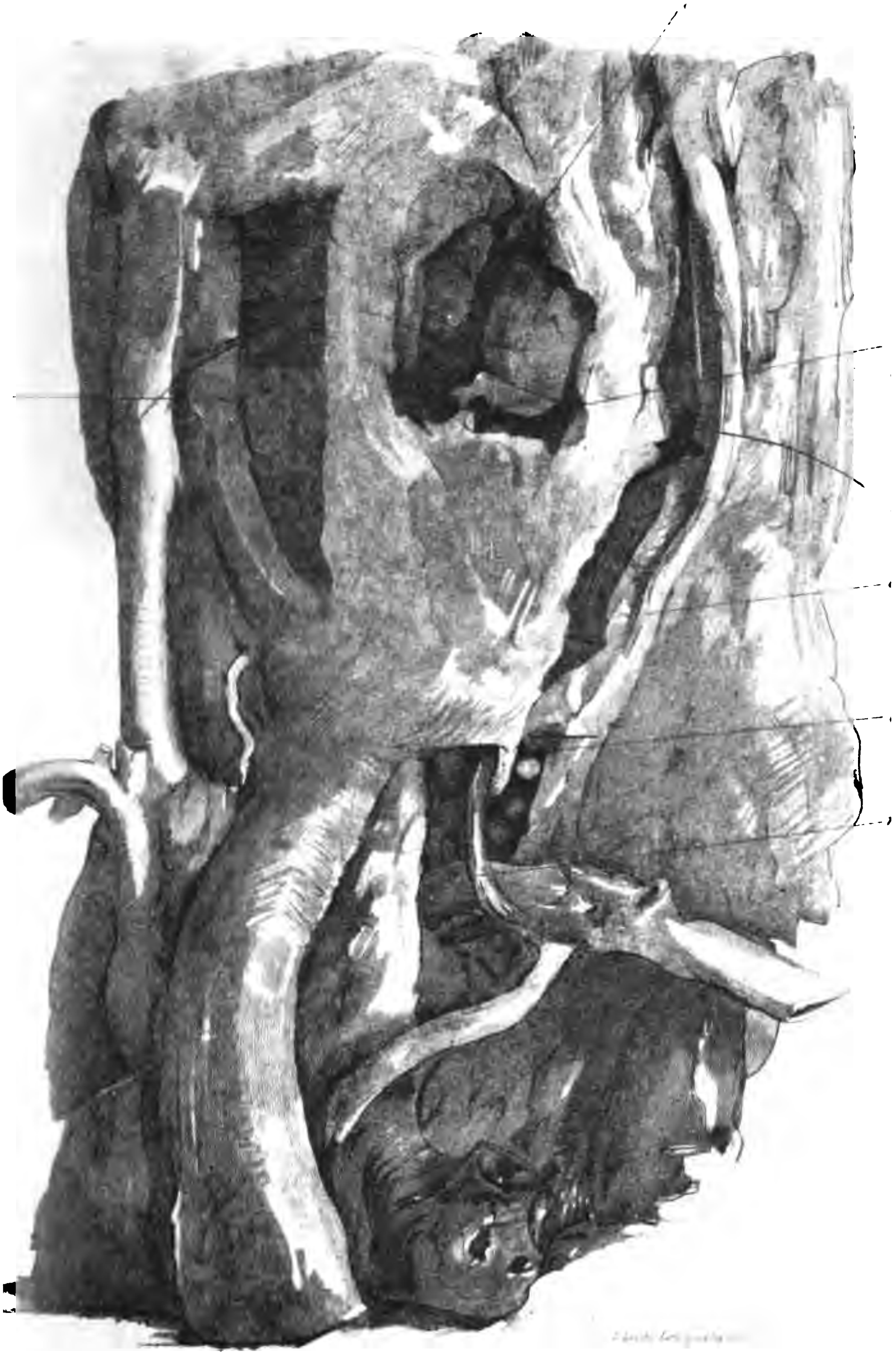
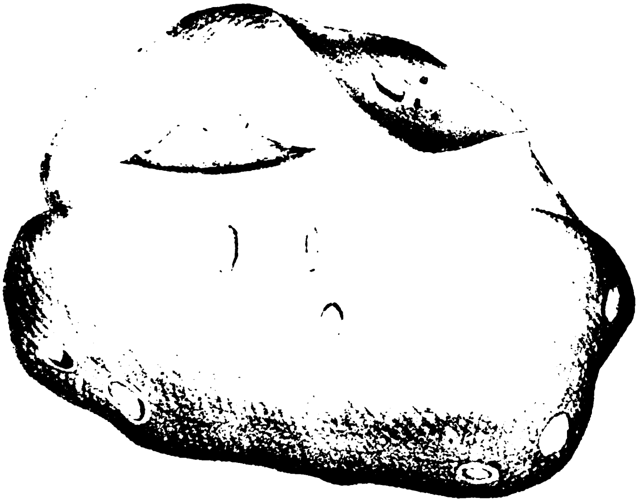


Dr DUNCAN'S CASE OF ANEURISM



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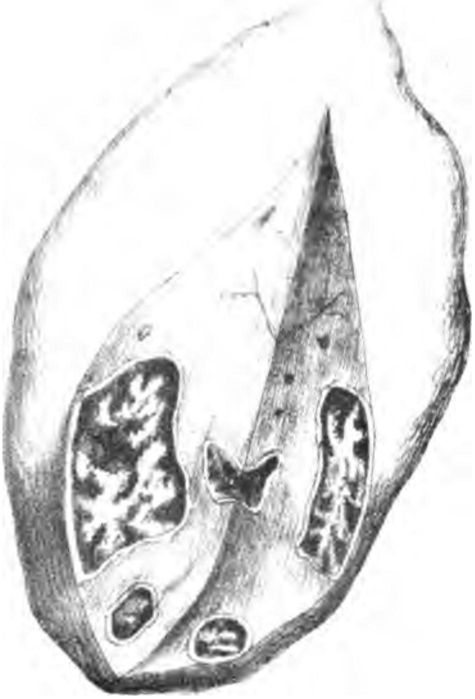


Fig: 2.

Dr Inmans Case

Fig: 2.

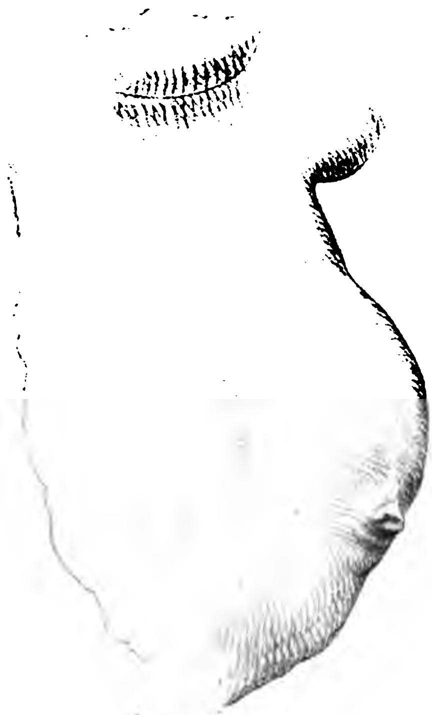
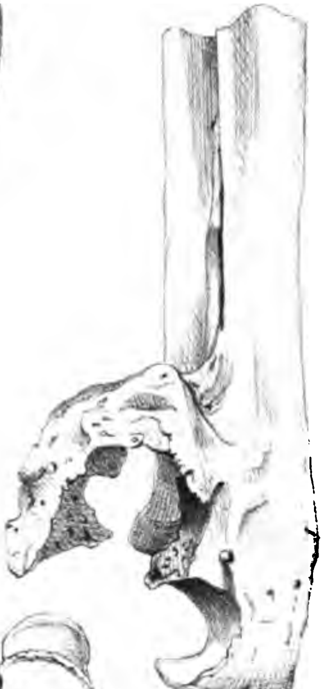


Fig: 3.



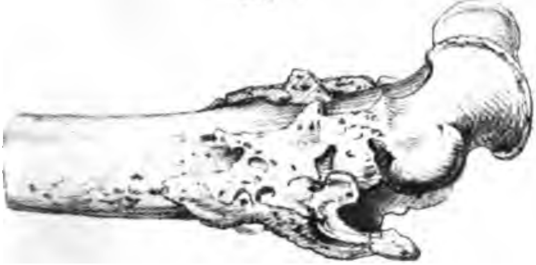
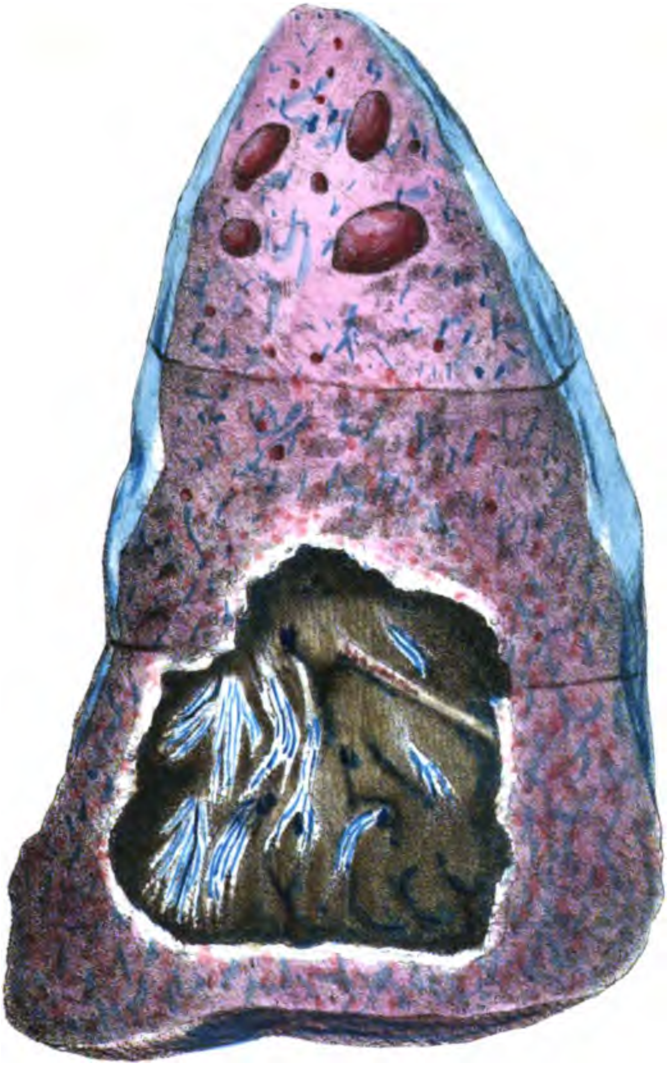


Fig. 1.



Dr Watson's Case of Gangrene of the Lung.

THE

EDINBURGH
MEDICAL AND SURGICAL JOURNAL.

1. OCTOBER 1844.

PART I.

ORIGINAL COMMUNICATIONS.

Art. I.—*Cases and Observations illustrating the History and Pathological Relations of two kinds of Hydatids, hitherto undescribed.* By Dr Gairdner and Mr Lee, *with Microscopical Observations* by Mr H. Goodsir.

1. *Case of Abdominal Disease, in which the substance of various viscera was destroyed by an entozoon, not hitherto described.* Read before the Medico-Chirurgical Society of Edinburgh, on the 1st of May 1844. By John Gairdner, M. D. F. R. C. S. E.

Early in 1843 I was consulted by a respectable tradesman, then in his 69th year. He complained of a deficiency of power in his lower limbs, affecting both of them equally; and also of some slight pain or uneasy feeling in his back, about the region of the lower dorsal or upper lumbar vertebræ. These, so far as I remember, were his only complaints. I was, at the time, of opinion that the loss of muscular power might be owing to some disorder, functional or organic, of that part of the spinal cord which corresponded with the seat of his uneasiness; and, in the hope that it was only functional, prescribed that he should apply towels dipped in very cold water to that part of the spine, evening and morning, for some minutes each time, and that he should afterwards rub the part with a rough dry cloth, till it was brought into a state of warmth and of vascular reaction. For reasons which will appear in the sequel, I greatly doubt the accuracy of the notions I then entertained of the theory of the morbid symptoms just detailed; but the patient decidedly improved under the treatment, the sluggishness of the limbs went entirely away, and with it the spinal uneasiness, and he repeatedly expressed himself much satisfied with the results of this very simple management.

In the month of July following, he again consulted me; and had then a new symptom, a considerable tumefaction of the abdomen, with obvious fluctuation.* No induration of the liver, nor any hard tumour could be detected in any part of the cavity. He complained of his bodily strength being a little impaired, and was quite obviously thinner than formerly. He was also somewhat sallower than usual, though not remarkably so, for he had naturally very little colour when in his best health. His urine amounted in twenty-four hours to about a quart; in colour and appearance it was little different from what it ought to be; when boiled, it presented no trace of albumen. He was remarkably free from pain.

The symptoms just detailed gradually increased. Diuretics of various sorts, and more especially squill, digitalis, mercury, acetate of potass, and nitrate of potass, were perseveringly employed, and excited only occasional and very inadequate diuresis. At first he was able to attend to his business and to go about the streets as usual, but towards the end of the year he became more and more debilitated, and very seriously incommoded by the great distension of his abdomen. His food was frequently returned, apparently in consequence of the pressure exerted on the stomach by the abdominal contents. I proposed to relieve him by paracentesis, which, after some delay, occasioned by his own reluctance to the proceeding, I performed on the last day of the year. The puncture was made in the *linea alba.* On withdrawing the trocar, not a drop of fluid followed it for some seconds. I passed a director through the canula, and a few particles of gelatinous-looking matter came away, followed by some drops of liquid. I adapted a syringe to the canula for the purpose of suction, with the aid of which I withdrew nearly a pound and a half of similar matter, and was then forced to give up, in consequence of the great difficulty of the proceeding, and of the probability of inflammation if it had been longer protracted. The diminution of the distension made a very considerable difference, for some days, in the poor man's comfort, and in his capability of receiving food; but he gradually became as bad as ever. I was therefore tempted to make a second effort to relieve him, with the hope that I might possibly be enabled to do so more effectually. I had not been prepared, on the 31st of December, to expect the difficulty which then occurred, and my apparatus for suction was consequently less perfect than I could have wished. When I repeated the paracentesis on the 18th of January last, I was provided with an excellent syringe for this purpose, and was therefore much disappointed to find that I was unable to get away more than ten ounces of the gelatinous matter. He was again greatly relieved for a short time, but again became gradually worse and worse. His death happened on the 14th February last.

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* The fluctuation, when carefully attended to, was somewhat peculiar in its character, approximating to that which is experienced on tapping with the finger one side of a vessel of animal jelly, while the other hand is applied to the opposite side of the vessel. The reason of this will be apparent from the history of the case.

I was permitted to examine the body the following day, about thirty-five hours after death.

The cavity of the peritonæum was found to be entirely filled with the same kind of matter which had been withdrawn by operation. It consisted of a number of masses, mostly of a globular or ovoid form, attached by pedicles (Plate V. Fig. 7,) to the inner surface of the peritonæum lining the abdominal parietes, and also to the peritonæal surface of the intestines and other abdominal viscera. The entire quantity of these masses amounted to about twenty-four imperial pints. Across the epigastric region extended a massy tumour of a dense consistence, which had been concealed during life by the great distension of the abdominal walls. Innumerable masses of the gelatinous-looking matter were attached to this tumour like bunches of grapes, (Plate V. Fig. 5.) When these were artificially separated from it, the surface from which they had been removed presented an appearance like honey-comb. It was at first difficult to say in what organ this dense tumour had been developed; but its connections with the surrounding parts proved it to be the omentum. The tumour was from two to three inches thick, very hard throughout, and, in some parts of it, almost of cartilaginous density. On cutting through it the stomach and colon were found imbedded in it, and firmly fixed together by it. The place usually filled by the right lobe of the liver was completely occupied by the gelatinous bodies. The lobe itself had entirely disappeared. The gall-bladder and biliary ducts still existed, and contained some bile. The common duct was traced to its usual termination in the duodenum. The left lobe of the liver was greatly diminished in size, but a portion of it remained in a tolerably sound state. The textures of the pancreas and of the spleen, though enveloped in the tumour already described, were not nearly so much invaded by the disease as those of the liver. The intestines had a very dark colour, resembling that of melanosis; they were much compressed by the distension of the belly, and had many of the gelatinous bodies attached to them. Besides those which were attached to the peritonæal surface there were others of small size in the subserous tissue in various situations, and more especially in the immediate neighbourhood of the bladder. The right kidney was natural; the left had a considerable tubercle within it, occupying that position in the organ where the secerning part of it passes into the uriniferous. The thoracic viscera were perfectly sound; the lungs rather unusually dark in colour.

The remarkable substances above mentioned deserve to be more particularly described. It is impossible to doubt that they were real entozoa of a peculiar kind, but they differed in many particulars from ordinary hydatids. They were contained in the general cavity of the peritonæum, instead of being developed, as hydatids are, within a peculiar sac. The fluid which they contained, and that in which they floated, were highly coagulable by heat, forming a firm curd resembling in all respects the white of a boiled egg, and, no doubt, of the same composition. The fluid of common hydatids, as we know from Dr Marcet, (London Med. Ch. Trans. Vol. ii. p. 376,) and from Dr Bostock, (Guy's Hospital Reports, No. V. p. 468–9,) is not coagulable by heat "except after very considerable concentration." (Marcet.) Ordinary hydatids are mere cysts or vesicles containing fluid; but the fluid of these bodies did not flow out when they were cut into, being retained, like the fluid part of the vitreous humour of the eye, and that with much more tenacity, by a multitude of septa, which were seen passing through them in various directions, and which, besides parting them into lobules, appeared to enter by innumerable minor subdivisions into the composition of these lobules, giving the whole a reticulated appearance. In colour and transparency they very exactly resembled masses of calf's-foot jelly. Their fluid was of a glutinous or mucilaginous consistence. A portion of it, subjected to the microscope, displayed a number of small cysts floating in it, obviously the germs of the large ones. The larger bodies, or hydatids, if they may be so denominated, were in some instances almost of the size of a hen's egg; and they were of all sizes, from that to the minute volume of the microscopic ones already mentioned. I have said that they affected the globular or ovoid form; but, either from compression, or other unknown causes, they were often very irregular and unshapely, and some of them were evidently broken and disintegrated. Many of the more dense among them had a degree of opalescence. The fluid in which they floated bore a very small proportion to the aggregate mass of these bodies themselves, and I have a suspicion that it was chiefly derived from the rupture of the larger ones. Their exterior surface was roughish, in consequence of the existence, at pretty regular intervals, of a great number of small points, which proved, under the microscope, to be so many minute discs, having in their circumference apertures or *stomata*, communicating with little tubes which ran from them in different directions in the outer coat of the hydatid. (Plate V. Fig. 8.) The external surface of these bodies was less obviously rough in proportion as they decreased in size, but even the smaller ones, under the microscope, shewed similar roughness. Almost all those which were not of merely microscopic magnitude were attached to some part of the textures. Many of them had short thick pedicles; some had pedicles resembling narrow tapes. A very inconsiderable force was generally sufficient to detach them. The few which seemed altogether free had probably been detached by some such slight cause. The adhesion of others was more tenacious, and many of them appeared to be generated between the peritonæum and the textures immediately exterior to it. I have said *appeared*, because the peritonæum was in very many places so altered as to make it difficult to say whether it really existed there any longer or not.

Those who examine the records of such cases cannot fail to remark, that very many histories are given of supposed hydatids, which were not hydatids at all, but merely such cysts as Dr Hodgkin has described under the title of "Adventitious serous cysts." (London Medico-Chirurgical Transactions, Vol. xv. p. 267.) These are developed in the ovary, mamma, testis, and many other textures and organs, with which they have a vascular connection; and such is the want of discrimination with which the term hydatid has been applied to them, that much attention is necessary to distinguish true cases of hydatids from false ones. There is also to be remarked in those narratives of real hydatids, which are to be found in the writings of physicians, a considerable want of perspicuousness, arising from the application of the term cyst sometimes to the hydatids themselves, and more especially the parent hydatids, within which the young ones are generally developed; and sometimes to those *vascular* capsules within which we usually observe the parent hydatids to be contained, much in the same way as a kernel is contained within a shell.* These vascular capsules, which are adventitious formations from the textures of the several organs in which hydatids are imbedded, I shall, to avoid confusion, term the *nidi* of the hydatids.

I shall now endeavour to discover how far the more distinctive points in this case resemble those of similar cases to be found in the records of past experience.

1*st*, The absence of *nidi* in the case related was one of its remarkable features. If the narratives of cases of true hydatids are read with care, it will be found that these nidi have been almost universally present. Among the fifteen cases of hydatids detailed by Dr Bright in the fifth No. of Guy's Hospital Reports, there are apparently none that were not contained in nidi. In his second case, nidi were found in the omentum, the liver, and the spleen. The excellent representation of the parasitic entozoa which accompanies his description, recalls strongly the appearance of those I lately saw. But they must have been essentially different; for they are described as mere vesicles containing a fluid "as pure as distilled water."

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* For a very accurate account of this appearance, I refer the reader to Mr Howship in Edin. Med. and Surg. Journal, vol. xliii. pp. 15-16.

2d. Extensive invasion of the textures of the liver by hydatids is by no means uncommon; but I cannot find many cases of *equally* extensive destruction of these textures with that which I have related. In Mr Howship's case, communicated to the Medico-Chirurgical Society of Edinburgh, (Edinburgh Medical and Surgical Journal, Vol. xliii. p. 13–22,) "the liver was diminished in thickness almost to the peritonæal coat; not more than about half an inch of its parenchymatous substance remaining." The hydatids "were connected together by extensive fibrinous adhesions." From this last expression, and from the description and delineation which follow, it appears clearly that they were contained in the usual *nidi.* They were the common acephalocysts, and therefore differed in structure from those which I saw.

A very remarkable case of the same sort is briefly related by Ruysch, (Observat. Anatomicæ, lxv.) in which he says that a bold and rude surgeon perforated the abdomen in the region of the liver, and discharged a great number of hydatids. A similar operation has often had a successful issue; but in this case the woman died; and it was found on dissection that "*tota substantia hepatis*, eo in loco, in hydatides degeneraverat."

Cheselden, in his Anatomy, relates (p. 212,) a similar case, in which "the gibbous part" of the liver was "*entirely wasted*," and contained hydatids in a proper nidus within that viscus. Another case will be found in Sandifort's Thesaurus, (Vol. i. p. 347,) and one in Meckel's Handbuch der Patholog. Anat. (Vol. ii. P. ii. page 409.) A large collection of such cases could easily be made, in not a few of which the hydatids were evacuated by incision during life from the site of the liver. In some, in which, it is to be presumed, the liver was not extensively injured, the health of the individual was restored.*

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* I would refer more particularly to an excellent case of this sort treated by Guattani, and reported by Lassus in the Journal de Médecine et de Chirurgie, Paris, an ix. vol. i. p. 137. Another excellent case is recorded by Dr Monro in his Morbid Anatomy of the Gullet, Stomach, and Intestines.

3d. I have not been able to find any case in which the omentum was morbidly altered in the particular manner which I have described. Dr Cooke, in his edition of Morgagni, (Vol. ii. p. 261, *note*,) has spoken of a *horny* state of the omentum from hydatids, which had fallen under his observation. This may possibly have been a similar case.

4th. The albuminous and highly coagulable character of the substance of the entozoon, is another point in the case which I have sought to illustrate from the records of medicine. I shall first of all produce a case of this sort from Bonetus (Sepulchretum, Tom. ii. p. 490,) who very pertinently inquires, "Quis medicorum curare potest hydropem asciten *limosum, membranis inclusum?*" He says he has heard and read of many such cases, and he gives the following as an example. "Aperui cujusdam virginis ætate provectioris cadaver, cujus admodum distenti abdominis totam cavitatem repletam conspeximus innumerabilibus vesicis aut sacculis *humore tenaciori et glutinoso* refectis." Here the fluid was glutinous and tenacious, and therefore very unlike that of acephalocysts; and yet, I fear, the case cannot be safely produced as an instance of hydatids, though so regarded by the author. For he does not say whether these "sacculi" were attached or not; and as the subject was a female, it is at least possible, if not probable, that they were serous cysts connected with the ovary.

The following case from Ruysch is not open to the same objections, and is, besides, one of the most remarkable for destruction of the liver which I have met with. I quote his own words. "Anno 1696 hydropici cadaver cultro anatomico subjeci, *cujus hepar, totum quantum erat, ex meris vesiculis constabat, quæ materiam limosam pellucidam continebant.—In dicto jecinore ne minimus quidem ramulus venæ portæ, cavæ, ductus biliosi, aut arteriæ hepaticæ videndum sese exhibebat,* quantumvis ad illud usque tempus vixerit æger." The author then enters into some speculations on the origin of the hydatids which are little to my present purpose, except on account of his characterising them as "*substantiam gelatinosam in se continentes.*" The patient was a male, as is evident from the first words above quoted; the disease was in the liver; the "vesiculæ" contained a matter which was not merely slimy and gelatinous, but *pellucid;* the description of these bodies, so far as it goes, would serve excellently for the appearances presented by the parasites in the case which I have described; and the extensive destruction of the liver completes the parallelism. (See Thesaurus Anatom., i. No. xii.) In a subsequent part of his writings, the author gives a plate representing a portion of this unusual liver. (See Thesaurus Anatom., ii. Table ii. Fig. 3.)

Dr Crowther has presented to the profession a remarkable enough case, in which considerable difficulty was found in tapping from the presence of hydatids. After the death of the patient, a female about thirty, the peritonæum was found greatly thickened; and presented, "on dissevering its laminæ," (such is Dr C.'s expression,) a great variety of "cysts, some round, some oval, some transparent, some opaque," the transparent ones containing "a thick, transparent, syrupy, gelatinous fluid," which is stated to have "coagulated by the application of heat." The abdominal viscera were all sound, the hydatids, if such they were, (and I think it probable that they were such,) being altogether external to the peritonæal cavity. There are few recorded cases in which the effect of heat on hydatids has been mentioned, and it is a pity that in this one, in which their coagulability was ascertained, the structure of the "cysts" is not more minutely described.* (See Edinburgh Med. and Surg. Journal, Vol. xxv. p. 49–50.)

5th, There yet remains one aspect of my case in which I have endeavoured to illustrate it from the observations of others; I mean, as it regards the external and internal structure, and the mode of attachment, growth, and development of the entozoon itself. I have endeavoured to give an accurate account of its characters in the former part of this paper, and my friend, Mr Harry Goodsir, conservator of the Museum of the Royal College of Surgeons, Edinburgh, whose familiarity with subjects of this description, so eminently qualifies him for the task, has examined it with great minuteness, and introduced the results of his inquiries into a paper which he has recently communicated to the Royal Society of Edinburgh, on the structure, economy, and development of the different species of entozoa. Mr Goodsir suggests ***Diskostoma Acephalocystis*** as an appropriate name for this species. I have resorted to all the most probable sources of information accessible to me, from which parallel cases might be expected to be adduced, and more especially to the express treatises on the natural history and structure of entozoa in general, or of hydatids in particular, by Rudolphi,† Bremser,‡ Cruveilhier,§ Kerr,‖ Owen,¶ Lobstein,**

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* A collection of cases will be found in Sandifort, 22 in number, in many of which the seat of the parasites was similar to that of those observed by Dr C. (Thesaur., vol. i. p. 451.)

† Entozoorum Hist. Nat.

‡ Traité Zoolog. et Physiolog. sur les vers intest. de l'homme.

§ Article "Acéphalocystes" in Dict. de Med. et de Chirurgie Pratiques.

‖ Article "Hydatids" in the Cyclopædia of Practical Medicine.

¶ Article Entozoa in Todd's Cyclopædia of Anatomy and Physiology.

** In his Traité d'Anat. Pathologique, t. i.

Meckel,[*] Laennec,[†] Lamarck,[‡] Copland,[§] Monro,[‖] and Hippolyte Cloquet.[¶] I have also examined those more incidental accounts of hydatids which are to be found in such pathological works as those of Morgagni, De Haen, Baillie, Abercrombie, and many others, some of which I have quoted in this communication: finally, I have examined a very great number of those still more casual descriptions which are connected with the details of individual cases in the transactions of societies, and in periodical journals, and other works. From all these sources I have not been able to extract a single description of a parasite at all resembling that which was fatal to my patient. I am therefore warranted in asserting that it is a rare one; and that if it has been seen, which it very probably may have been, it has at least never yet been described with that precision which is necessary to its identification.

2. *Description of the Hydatid found in Dr Gairdner's Case.* By HARRY D. S. GOODSIR, Conservator of the Museum of the Royal College of Surgeons, Edinburgh.

THE hydatids found in this case were globular, of various sizes, (Pl. V. Fig. 7, also preparation 2241, Mus. R. C. S. E.,) and were connected together in groups or masses, each group resembling the ovaria of birds in an active state of development; they were of a bright straw-colour. Each group was covered externally by a membrane, (Pl. V. Fig. 8,) which was formed by the hydatid. At first this membrane, from its appearance, was thought to have been the result of inflammatory action, but a more minute examination proved it to be a tissue *sui generis*, and one which intimately belonged to the hydatid. Its external surface was rough, in consequence of a number of striæ, which ran at irregular intervals across one another, so as to form interspaces of an irregular angular shape, (Pl. V. Fig. 7.) This membrane not only covered the surface of the group, but also dipped deeply between the hydatids and in those specimens which were altogether isolated, the membrane covered them entirely, even to the base of the pedicles, and generally passed from the hydatids to the surface of the peritonæum, where it became gradually thinner and thinner, as it receded from the parent stock.[**]

When magnified this membrane presented a very peculiar structure, (Pl. V. Fig. 5); numerous disks of various sizes were seen scattered over its external surface, and larger disks were sometimes seen with two or three smaller ones upon them. Numerous tubuli ramified through the membrane, and opened on its surface, by means of small stomata, which were generally arranged round the edges of the disks, (Pl. V. Fig. 8.) Immediately underneath the membrane already described, there was a layer of cellular substance which connected it with another of a much more delicate texture. From observations made upon another, and also new form of acephalocyst,* I have been led to suppose that a third membrane exists in this hydatid, which probably lies internal to the two others, and from which the delicate septa which traverse the gelatinous mass of the body arise.† The stomata and tubes which were observed in the external membrane could not be traced into the deeper parts of the body of the animal.

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[*] In his Handbuch der Patholog. Anat. v. ii.

[†] In the Bulletin de l'Ecole de Médecine, &c. de Paris, an xiii.

[‡] Histoire Naturelle des Animaux sans Vertèbres, t. i., Paris, 1816.

[§] In his Dictionary of Practical Medicine, Art. "Hydatid."

[‖] In his Morbid Anatomy of the Human Gullet, Stomach, and Intestines.

[¶] In the Dictionnaire de Médecine, Articles "Acéphalocyste," "Cysticerque," "Polycéphale," and "Ditrachyceros."

[**] Transactions of R. S. E., Vol. xv.

When a small portion of the external or tubular membrane was placed under the microscope, its internal surface was found to be studded with numerous small shining bodies or cells. In general these cells were compound, containing from one to four, or more young cells in their interior.‡ These I looked upon as the gemmules of this hydatid, which, like the other acephalocystic entozoon, is gemmiparous. The younger or contained cells were sometimes seen free and detached from the parent cell, floating in the gelatinous contents of the parent hydatid. The tubular membrane, as it extends over the peritonæal surface, after arriving at a certain stage of growth, throws off cells similar to those just described from its attached surface, and invariably from spots in the neighbourhood of the large tubes.§

These cells enlarge, not by any apparent cellular development, but, like those of the common acephalocyst, by simple dilatation from the addition of new matter in the interior. As this increases, the tubular membrane in the neighbourhood becomes thicker, owing to the increased number of tubes necessary for the nourishment of the young cell.

In this entozoon there are two modes of propagation, one for increasing the size and extent of its own individual group; the other for the purpose of extending the species to as yet uninfested portions of the infested animal.

The first mode of propagation we have just described; the second would appear to proceed in the following manner:—The gemmules, by some means or other, escape from the parent acephalocyst, reach the uninfested tissues at some distance from the general parasitic mass,‖ and thus in time form new groups, which,

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[*] Vide, p. 279.

[†] Transac. R. S. E., vol. xv. p. 563.

[‡] Ibid.

[§] Ibid. vol. xv. pl. xv. fig. 2, C.

[‖] Ibid. p. 564, pl. xv. fig. 2, D. E. B.

in their turn, throw off gemmules also. This goes on for a longer or shorter period, until at length the whole surface of the peritonæum is completely occupied and covered. So soon as this is effected the succeeding gemmules are forced into the deeper tisues. As they increase in size, they tend always towards the surface of the infested cavity, and at length burst from their confinement, adhering, at the same time, to the bottom of their former, containing cellules by pedicles, (Pl. V. Fig. 5.) In this manner a peculiar honey-comb appearance was produced in consequence of the breaking up of the tissues surrounding them, (Pl. V. Figs. 5 and 6, also preparation 2242 in Museum of the Royal College of Surgeons, Edinburgh.)

3. *Case of Ascites connected with a peculiar Parasite within the Peritonæal Sac.* (Read before the Medico-Chirurgical Society of Edinburgh on the 5th June.) By Thomas M. Lee, Esq.

Jane Robertson, aged 53, had been twice married; first fifteen years ago, and a second time six years afterwards; had had two children, both by her first husband; the elder died while teething, the other is now 12 years old. She had lived in Edinburgh till she was last married, but since then had resided in different parts of the neighbouring country, wherever her husband, who is a labourer, could obtain employment, but had returned to town about fifteen months before her death. Her first husband died of cholera when that disease prevailed in this city. Her father and her only sister died paralytic, and her mother died of consumption. She had two brothers, one is still alive, the other died when a child of small-pox, or soon after having that disease. She is said to have been remarkably stout and healthy up to the time of her second marriage, soon after which her health began to decline, and since then she has had to endure many privations, and has suffered much from mental anxiety caused by domestic differences. She at first complained of drowsiness, and soon afterwards began to lose flesh and colour.

About five years ago, after labouring for some time under a disease of the knee-joint, which, however, yielded to repeated blistering, she had a spitting of blood, which left her very weak; but it does not appear that she was troubled at any time with cough. She has apparently had more or less of ascites for the last three or four years, and during that time has complained of a disagreeable sensation at her stomach, as if there were something in it, the removal of which would have given her relief, and of an inclination to vomit after eating, along with a dread at being touched about the abdomen. I saw her for the first time on the 19th of February last; the symptoms at that time were, anorexia, globus, a bruised feeling along the lower border of the ribs, pain in the back, tenderness in the left hypochondriac region, pain at stomach immediately after food, with sweetish regurgitation, bread and tea being the sort of nourishment after which she experienced the least uneasiness: the abdomen was considerably distended, and fluctuation could be felt all over it; percussion on the abdominal parietes, while she lay on her back, produced a dull sound except over a small space in the umbilical region. She occasionally sweated at night after rigors, and had sometimes a disagreeable feeling of heat about the soles of the feet. She had been troubled with bowel complaint, but not for the last six weeks; her pulse was 80, her tongue clean, and her bowels regular; her urine, of which she made scarcely four ounces daily, was sometimes passed involuntarily, and was not coagulable by heat. During the next week, I have marked the state of the pulse once, when it was 68; the urine increased to twenty-four ounces under the use of digitalis, squill, and blue pill; she continued to take a blue pill every night until she had taken nine, when her gums became slightly affected. On the 26th of February she was a little feverish, and had had some shivering the night before; her skin was hot; tongue clean; pulse 88; urine twenty ounces. On the 1st of March she complained of a *jagging* pain at the epigastrium, of a feeling as if something heavy fell in her abdomen when she turned, and of a hot ball rising up her throat; she had no appetite and no thirst; her tongue was clean; pulse 68, of fair strength; skin cool; urine ten ounces. On the 11th of March, her mouth, which had been very slightly affected by the mercurial pills for about a week, was well; she still complained of pain, or rather heat after eating; a good deal of pain was excited by pressure on a spot a little below and to the right of the ensiform cartilage; no tumours could be felt in the abdomen; her bowels were regular, but her urine was very scanty and drained away involuntarily, and was said to have a very disagreeable odour; she complained of downbearing, which she had had sometimes before to a very unpleasant degree; she had had no discharge of any kind from the vagina for many years, but had sometimes pain in the region of the uterus and bladder; the *cervix* and *os uteri* felt healthy, but an irregular, hard, and moveable body, about the size of a hazel nut, could be felt through the upper part of the anterior wall of the vagina; (at a subsequent examination two or three of these bodies were felt in this situation); the uterus was not very moveable especially on the left side, and the lower extremity of the cervix was inclined to that side. March 15, had vomited some dark-brown fluid having a sweet taste, which is said to have resembled strong tea, and afterwards had lancinating pain in the right shoulder; tongue clean; pulse 60, of good strength; stated that she never had any headach or bad taste of mouth. On the 25th of March, she still complained of heat and ball in the throat, abdominal distension had increased; her feet were a little swelled, and pitted when pressed; she felt very weak, but thought she would be well enough if she could live without eating. On the 20th of April she had retched much the night before, and had vomited a little fluid resembling thin mucilage with a little snuff disseminated through it; her tongue was slightly furred, and she complained of pain to the right of the epigastrium; her skin was warmer, and her pulse firmer than usual; she had taken a little wine a few minutes before I called. On the 21st she vomited some more of the brown fluid, and complained much of pain; her pulse was 76, of fair strength, and her tongue was white. After this she was kept pretty easy by large doses of morphia, but sunk rapidly, and died much emaciated on the 11th of May. For the last three weeks of her life, her bowels, which, for sometime had been getting less regular, were almost unmoved,—enemata, which could be administered only in very small quantities, having had a slight effect but once during that time; she had a good deal of thirst, but whatever she swallowed seemed to stick at the lower part of the œsophagus until it was rejected; for the last four or five days, however, she could swallow water freely. The conjunctiva became inflamed a few days before her death. Her mind continued calm to the last.

Dissection, May 13.—Present, Mr John Goodsir, Mr Harry Goodsir, Dr Gordon, and myself. When the cavity of the abdomen, which contained four or five gallons of a greenish yellow fluid, having an oily or ale-like consistence, was exposed, none of the abdominal viscera could be seen, except part of the colon, distended with flatus, and projecting over the spine between the kidneys. The intestines were found to be firmly bound down to the posterior part of the abdominal cavity, by a mass of gelatinous-looking matter of a structure somewhat similar to that of cooked tapioca, interspersed with irregularly rounded cysts of various dimensions, one or two fully three inches in their longest diameter, but for the most part about the bulk of the largest sizes of small shot. The peritonæum of the abdominal parietes was also studded with the same bodies, the larger ones being attached by a peduncle generally so slight, that when they were pulled away no trace of their attachment could be distinguished. The larger and many of the smaller cysts were covered by a strong membrane, of a dingy colour, having somewhat the appearance of intestine, but sufficiently transparent to allow of its being seen that they were filled with other cysts of a smaller size. One of the largest of these cysts was found loose in the abdomen, and one or two of a smaller size, which were also loose, were probably broken from their attachments by my hand coming against them. The smaller cysts were apparently attached more firmly to the peritonæum than those of a larger size. A considerable part of the peritonæum was also covered with a yellowish gelatinous-looking matter, of a globular structure, but without the dingy membranous covering, similar to the contents of the larger cysts. The intestines were so firmly matted together, that an accurate examination of them could not have been made without more time and care than the circumstances would allow; hardened fæces were found in some parts of them, and the vermiform process was inverted, and lay within the gut. The uterus, exclusive of the cervix, was not much larger than a good sized filbert, and lay imbedded in a mass of the gelatinous matter, which was impacted in the pelvic part of the abdominal cavity, and which apparently prevented the bladder from contracting. The peritonæum was a little wrinkled in some parts, from the effusion and subsequent contraction of lymph, doubtless the result of the irritation caused by the presence of the parasite. No omentum could be seen. The liver, which was bound by a firm layer of gelatinous matter to the back part of the diaphragm, was small, flattened, and of a soft consistence. The stomach was apparently healthy; but some patches of ulceration were observed on the mucous lining of the lower part of the œsophagus. The only kidney examined was small, but was not remarked to present any morbid appearance. The substance of the heart was soft, and there was some slight osseous deposit about the aortic valves. There were some calcareous tubercles at the apices of both lungs, but they were otherwise healthy.

4. *Description of the Hydatid found in Mr Lee's Case.* By HARRY D. S. GOODSIR, Conservator of the Museum of the Royal College of Surgeons in Edinburgh.

This acephalocyst is very different from the *Acephalocystis simplex* and the species already described under the name of *Diskostoma acephalocystis.* It differs, however, from the species alluded to in points of considerable importance, and of such a nature as to require the constitution of a new genus for its reception.

It will be remarked, by those who are acquainted with these animals, that the *Acephalocystis simplex* is much less complex in its structure than the Diskostoma, on which account there can be no real affinity between them, a connecting link being awanting. In the acephalocyst under consideration we have what is desiderated; and it will be the object of the following remarks to point out its structure, functions, and relations.

In its rudimentary state, the *Astoma acephalocystis* is of a globular form; but in the parent this form is to a great extent lost, owing to the young cysts within it increasing irregularly in size, and presenting a nodulated appearance. In the larger specimens, the external surface is quite smooth, an appearance which seems to arise from distension, the small or young ones being rough, and corrugated externally. The parent cyst is very strong, fibrous, and dense, and is composed of three membranes, the outermost of which is the strongest, the *remaining two* being much more attenuated, though at the same time of considerable strength. On stripping the external layer from off the two others, and placing it under the microscope, it presented a peculiar reticulated appearance, with a few indistinct masses of ovules on its internal surface, (Pl. V. Fig. 1,) separated by accident from the succeeding or middle layer of membrane, which appeared to be the seat of developement of the gemmæ. At the same time the internal membrane and the septa which arose from it, and ran into the gelatinous body of the hydatid, appeared to be matrices for these cellules. The structure of the middle membrane was more fibrous than the external one, (Plate V. Fig. 2.) The innermost of these three membranes was very peculiar in its structure, and differed altogether from the other two. When placed under the microscope it was transparent and colourless, with an immense number of fibrous-looking bands running through it in all directions, which ramified in a manner similar to the extreme capillaries of blood-vessels. A number of ovules were attached to this membrane, but there were few when compared with those in the middle one. In this membrane also numerous reticulated circular masses of considerable size were seen. These were the ovules in an advanced state of growth, (Plate V. Fig. 3.)

The great mass of the body of this animal consisted of the same gelatinous-looking substance of which the Diskostoma was composed, but it was not in such abundance. At the same time numerous septa or fibrils, similar to those observed in Diskostoma, traversed the gelatinous mass. Several distinct ovules were seen attached and apparently growing from the septa in this part, and in the jelly a number of small filaria was frequently observed, (Plate V. Fig. 4.) The middle part of the body of these thread-worms was rather large, and tapered gradually to so fine a point from this to the extremities, that the eye lost all trace of them even under a strong power. The middle part of the body was black, but both the extremities were colourless. The filaria were sometimes seen single, but generally they were lying in masses, twisted, and contorted with one another. They were much more common in some specimens of the acephalocyst than in others.

The free or parent Astoma varied in size from a millet-seed to that of a large orange, and was of a lightish brown or sand colour. Those that were not larger than a filbert were globular; as soon, however, as they became larger, they assumed the nodulated appearance, which gradually increased with the size of the animal.

When a small portion of the middle membrane was put under the microscope it was found, as already observed, to be filled with small ovules, most of which were of a similar size. Some of these, however, were observed to be larger than others, but it was not till they reached, or came in contact with the inner membrane that the ovules began apparently to increase much in size. As the ovule increases in size it projects into the cavity of the parent cyst, sometimes carrying before it a layer of the internal membrane of the parent; and it is to be observed that, as this process is going on, the secondary animal, or hydatid, is also being distended with young, which again throws off another or fourth series, so that there is a constant formation of young,—one within the other, the parent or original cyst containing all the formations within itself (Plate VI. Fig. 1, preparation 2243, Museum Royal College of Surgeons.) When a parent cyst, therefore, is divided, the internal structure will be found to be very complicated, consisting of cells within cells of all sizes. The parent cyst, after arriving at a certain state of distension, bursts, whereby the young, which had been formerly confined within it, are exposed, and as the original cyst is now useless, it is absorbed, the young in the meantime becoming attached to the peritonæum. (Plate V. Fig. 3, also preparation 2244, Museum Royal College of Surgeons.)

In this species, as in the Diskostoma, there are two modes of propagation, viz. one for the extension of the group, and another for the extension of the species to the portions of the body as yet uninfested. The former of these, which takes place at the expense of the parent cyst, is that which we have just described, the latter is exactly similar to the analogous mode in the Diskostoma. The younger cells, which have become detached from the membranes, and are seen suspended in the gelatinous fluid, escape, by some means or other, from the parent hydatid, and reach the parts of the body which have hitherto remained uninfested, where, forming a nidus, and, gradually increasing in size, they become independent animals, and in time throw off ovules also. (Plate V. Fig. 2.)

This species, like the *Acephalocystis simplex,* is without any apparent organs of nutrition, unless the ramifications which are seen very slightly in the external membrane, and more strongly in the two internal coats, are looked upon as vessels,—a point which cannot be determined with accuracy.

It was suggested by Dr Gairdner that the cysts might be supplied with blood from the body of the infested being, which would, he thought, prove their affinity to the morbid and adventitious cysts found in the ovaries and other parts of the body. With the view of deciding this point, I had the vessels of a large portion of the intestines, which was infested with the hydatid, filled with minute injection, when I found that, although the intestines were most minutely injected in all parts, the hydatids which were attached to them were perfectly free of blood-vessels. In many instances, also, the peritonæum, round the base of the parasite, where we would naturally look for inflammatory appearances, was found to be most minutely injected, while there was not the slightest vestige of injection on the parasite itself. (See preparation 2245, Museum of Royal College of Surgeons, Edinburgh.)

The propriety, however, of classing these bodies along with the Acephalocysts, or even looking upon them as having any degree of affinity, has been questioned. In a paper read before one of the late meetings of the Royal Society, I proved that the common Acephalocyst and Diskostoma were independent animals.* Now, the Acephalocyst under consideration, as regards both its structure and functions, has a great affinity to Acephalocystis as well as Diskostoma, so that if the animality of these two is granted the like must be granted to Astoma also.

In investigating the nature of adventitious cysts, and comparing them with the bodies under consideration, it cannot but strike the observer, that a remarkable difference exists between them. In the former, we have a thick, highly-vascular coat, with a number of large vessels permeating it; the minute structure of which consists always of apparently compressed cellular substance, being in this respect exactly similar (and formed in a like manner,) to the vascular coat which surrounds the external cyst of the Acephalocystis, which has no connection with the hydatid, being entirely composed of the tissues of the infested person. In the latter, the minute structure of the tissues is very different and decidedly non-vascular. The mode of propagation, however, is perhaps the most specific mark of difference which we have for the distinguishing of these two very different bodies.

Art. II.—*On the Nature of the Nervous Agency.* By James Stark, M. D., F. R. S. E., Fellow of the Royal College of Physicians, Edinburgh, &c.

The nature of the nervous agency is one of those problems which have engaged the attention of philosophers in every age, but relative to which modern research has left us as much in the dark as ever. In proportion as the phenomena of electricity and galvanism ha vebeen better understood, but more especially since the discovery in certain animals of organs which are capable of generating electrical fluid, it has become the fashion to attribute nervous agency to the conduction of a fluid identical with or analogous to the electric or galvanic. This theory, so specious in itself, and apparently supported and almost demonstrated by numerous experiments, overturned the old theory of the circulation of animal spirits, or of a serous fluid in the nerves; and is at the present moment so generally received, that I know not a single treatise which takes an opposite view of the matter. Longet, who published an able treatise on the nervous system in 1842, goes further than any previous writer in endeavouring to show that the experiments as yet published do not prove the identity of the nervous and electrical fluids, but he goes no farther, nor does he attempt to substitute or hint at a theory which would ageee more closely with ascertained facts. I was not aware of the existence of Longet's work till this year, (March 1844,) but in 1842 I transmitted a paper to the Royal Society of London, challenging the electrical doctrine, and substituting one which seemed capable of explaining all the phenomena. As every experiment which has been published since, with the view of throwing light on the nature of the nervous agency, tends more and more to confirm the conclusions at which I then arrived, and as no other writer, so far as known to me, has suggested any theory to supersede the electrical one, I shall endeavour to detail my views on the subject.

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* Transactions, Royal Society of Edinburgh, vol. xv. p. 561.

Almost all writers on the nature of the nervous agency have begun at the wrong end. Instead of ascertaining what was the composition of the nerves, and whether the substances of which they were composed really possessed the power of conducting the supposed electrical fluid in a manner superior to the other tissues of the body, they commenced by cutting nerves across, and from finding that electricity or galvanism applied to the cut extremity produced contractions in the organs to which the terminations of the nerves were distributed, jumped at the conclusion that the nervous fluid must be electrical or something analogous thereto. Another set of experimenters endeavoured to show that the nervous centres were generators of electricity, and from finding that a certain amount of electricity was manifested when these organs were made into a galvanic pile, also inferred that they had at last resolved the mystery, and proved to demonstration that the nervous agency was electrical.

In my investigations I followed a very different plan; and accordingly, when I commenced my researches into this subject in 1841, I began by endeavouring to ascertain what was the chemical composition and minute structure of the nerves themselves. As it appears to me that many of the results at which chemists had arrived, relative to the composition of the nervous system, arose from their using agents which acted chemically on the nervous matter, so that they procured products, not educts, I endeavoured to avoid such an error in my first experiments.

A piece of nerve carefully cleaned from surrounding filamentous tissue, &c., was simply dried. It shrunk considerably, and its surface became bedewed with a clear fluid, when the weather was warm, but the fluid became granular or congealed at ordinary temperatures. Between the fingers this fluid felt unctuous or oily, had the odour of animal oil, and a fatty taste. Some of this fluid was collected in a fine glass tube, when it was found to furnish all the characters of ordinary animal oil. It was clear and transparent when heated; at ordinary temperatures, however, it assumed the form of a yellowish-white, soft-solid granular mass —very much as animal oil or soft butter does in similar circumstances. It formed a soap with alkalis, from which acids separated the usual fatty acids. It produced a greasy stain on paper, and burned with the characteristic flame and odour of fat or oil. No doubt could therefore remain that the fluid which oozed from the nerve was true fatty or oily matter; and as no agent had been employed which could have produced this result by a chemical decomposition of the nervous structure, the only conclusion which could be drawn, was, that oil constituted one essential portion of the nervous structure.

The dried nerve was now carefully wiped by, and pressed between blotting-paper so as to remove all adhering oily particles, when it presented the appearance of a shrivelled tendon. It could be torn off in long parallel fibres or threads, which seemed to have a strong lateral cohesion to each other. On washing it with sulphuric ether, in order to remove all remains of fatty matter, and subjecting it to various re-agents, it was found to present all the characters usually recognised to belong to albuminous matters.

This simple analysis, then, showed that nerves were composed essentially of oil and of albumen. No attempt was made to ascertain the presence of phosphorus or of salts, seeing that in so far as the determination of the nature of the nervous agency is concerned, all that appeared essential was to ascertain the leading *animal* (or organic) ingredients of the texture.

Nerves were next analyzed in the more usual way. A piece of nerve was macerated in sulphuric ether, and several times washed with the same fluid. When the solution was evaporated, a fatty matter was obtained, which presented all the ordinary characters of animal fat. It differed somewhat from the fatty matter obtained by simply drying the nerve, in being of a more solid consistence, and requiring a greater degree of heat to melt it. This was a change which had probably been produced by some chemical action of the ether on the fatty matter. The animal matter left untouched by the ether had still the appearance and structure of the original nerve. It could be torn off in fine parallel threads; and when dried became nearly transparent, and had the aspect of a tendon. On being macerated in water it again resumed its white colour, in fact, comported itself exactly as tendon or animal membrane. On a more minute analysis it was found to be composed chiefly of albuminous matter.

The next object was to ascertain the mode in which these two principles were united so as to constitute the nervous fibril. The above experiments showed that the nerves were fibrous, and that even when deprived of their oily matters the general form, structure, and fibrous appearance remained unaltered. Drying the nerve showed, moreover, that the oily matter escaped from the surface of the fibre by a kind of oozing. From these circumstances it seemed reasonable to infer that the fibres consisted of hollow tubes, which in their natural state were filled with the oily matter. To ascertain this point it was requisite to employ the microscope, and as I well know how apt observers are to be deceived by trusting to observations made with one form of instrument only, nerves in their recent and dried state, nerves after maceration in ether, after having been dried and then macerated in water to re-swell them, were examined by means of simple lenses of high powers, of crystal and garnet, by the common compound, and by the achromatic compound microscopes.

It was then seen that nerves were composed of parallel longitudinal hollow fibres or tubes, which were in every case perfectly cylindrical, or of equal diameter throughout all their length. It signified nothing what nerve was examined; the same structure was found present in all. By examining the nerves prepared in the above-mentioned various modes, all fallacy was avoided as to the exact shape of the tubes; and had Ehrenberg and others, who assert that the nerves of sensation have regular dilatations at certain intervals, used the same means, they would have avoided making such a statement. The appearance of dilatations at intervals is produced by the handling; the granular oil being pressed away from some portions and swelling out other portions of the tube. There were two modes of easily proving the dilatations to be produced by the handling. 1. On passing a needle along the fibres, the granular and fluid oil was pressed before it' and dilated the appparently contracted portions of the tube to the same diameter as the other portions, showing the whole tube to be cylindrical. 2. On examining a portion of nerve which had been dried after it had its only matter removed by ether, but was again restored to the soft state by maceration in water, the water was seen to have entered the whole of the tubes by capillary attraction, and to have dilated them equally throughout, and to have occupied the place of the oily matter removed by the ether.

These cylindrical tubes then were filled with the oily matter which had been procured in the one experiment by drying the nerve, in the other case by means of maceration in sulphuric ether. When the weather was warm and dry, and a fine portion of nerve was examined for a considerable time, the tubes were distinctly seen to become shrivelled, and contract, when the oily contents escaped from their ruptured extremities, and formed drops, (which often coalesced,) at the mouths of the vessels, and also escaped from the interior, through the walls, by a kind of oozing, as the surface of the tube became dry. The high refractive power of these drops shewed them to consist of oily matter. By cooling the plate of glass on which they lay they became granular or solid, and by heating it again melted, when several of the drops would often be seen to coalesce.

The more minute observations on the microscopic anatomy of the nerves I must reserve till some future opportunity, as all I wish in the present paper is to demonstrate so much of the structure of these organs as shall throw light on the nature of the nervous agency.

Nerves, then, are simply composed of fine membranous tubes filled with an oily fluid. By examining the nerve in an animal recently killed but still warm, and heating the plate of glass on which it lay to the same temperature as that of the animal's body, it was seen that the oily matter was quite fluid within the membranous tubes; but as the plate cooled on which the nerve lay the oily matter assumed a granular appearance, in consequence of becoming more or less solid. In cold-blooded animals the same still was true. During the life of the animal the contents of the nervous tubes were invariably fluid. In cold weather, however, when the animal was more or less torpid, a somewhat granular state of the nervous oil was more than once observed.

Having thus arrived at something approaching an exact knowledge of the structure and composition of the nerve, we are prepared to enter into the investigation of the nature of that hitherto unknown influence which we know must be propagated along these organs, in order to convey volition to distant parts of the body, and sensation to the nervous centres. So long as the true structure and composition of the nerves were unknown, all inquiries into the nature of the nervous agency rested on pure theory. It was a just conclusion, therefore, which Dr Roget, one of our latest and best writers on physiology, drew from the present state of our knowledge on this very subject, that "we are completely ignorant of the nature of that power by which nerves effect that rapid communication along the lines of their fibres, and even of the changes which take place in the nerve while it is performing this function."* But Dr Roget is not alone in arriving at this conclusion; for every one who has impartially examined the whole subject in all its details must confess with him and Longet that we are as far at sea as ever, relative to the nature of the nervous agency.

Of the various theories which have been started from time to time relative to the nature of the nervous agency, that which has superseded all the others, and, indeed, is the only one at all considered in the most recent works on the physiology of the nervous system, is that "the agency which is transmitted along the nerves is of a galvanic or electric nature, or an agent similar to them." Let us see, therefore, how this theoretical assumption accords with experiment, and with what I have ascertained to be the true structure and composition of the nerve.

Are Nerves Conductors of Electricity?—The experiment of Wilson Philip of finding that food was digested in the stomach of an animal though the eighth pair of nerves were cut across, provided the cut extremities were connected with a galvanic apparatus, proved to the satisfaction of many both that nerves were conductors of electricity, and that the nervous influence was identical with or similar to the electrical. MM. Breschet and Milne Edwards in 1825,† M. Braschet‡ in 1837, and M. Longet§ in 1842, by performing the same experiment with various modifications, satisfactorily proved that the same results could be obtained by simply irritating by mechanical means the cut extremities of the par vagum,—a fact which proved that electricity, at all events, had nothing to do with it, and that nervous and electrical agencies were far from alike.

The same conclusions are arrived at from a review of the experiments which attempt to prove that electricity can supply the nervous energy by producing contractions of the muscles. Every one must have recognised that mechanical irritation of the nerve which is distributed to a muscle causes that muscle to contract as rapidly and powerfully as when electricity is itself applied, and also that it is of no consequence whether the irritating body consists of a conductor or of a non-conductor of electricity. This I have myself repeatedly ascertained. The satisfactory experiments of Longet, however, have distinctly proved that muscles retain their irritability and power of contraction for weeks after their nerves have been cut across, ***and for weeks after their nerves have lost their power of conveying the irritation of electricity or of mechanical agents.****

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* Encyclopædia Britannica, last Ed. Vol. 17, p. 672.

† Archives Générales de Médicine, Vol. vii. p. 197.

‡ Recherches Experimentales sur les Fonctions du syst. Nerv. Ganglion. Paris, 1837, p. 250.

§ Anatomie et Physiologie du Systeme Nerveux, 2 vols, 8vo. Paris, 1842, Vol. i. p. 124, and Vol. ii. p. 322.

Perhaps the electrical theory has received the strongest support from the known fact of the development of electricity in certain organs attached to a few fishes. It is well known that these organs are largely supplied by nerves, and it has been inferred that the electrical apparatus served simply the purpose of a Leyden jar, to collect the electricity transmitted there from the nervous centres. Few satisfactory experiments have been made with the view of determining the site of the production of the electricity in the electrical fishes. It is known that when the nerves going to the organs are cut, the animal loses the power of giving shocks, and M. Matteucci has lately ascertained that tying the nerves has the same effect. This fact, therefore, of itself shows that the nervous influence, whatever it be, cannot be anything analogous to the subtile electric or galvanic fluid, else simple compression of the conducting body could never prevent its transmission. The experiments of Person† in 1830, fully demonstrate this statement. He found that though compression, or a ligature, or section of a nerve completely prevented the transmission of the nervous agency, electricity could be transmitted through it as readily as before, as readily as if it had consisted of membrane. We know, besides, that the electrical shock from these electrical organs is given off in one direction only, (a fact also lately authenticated by M. Matteucci)—a circumstance which indicates that the electricity is actually generated within the electrical organs themselves, which, in fact, in the arrangement of their parts, and in the nature of the fluids and solids which compose them, shows that they act the part of a galvanic pile. The probable use of the large nervous supply seems alone to be to put these organs more perfectly under the control of the animal, and enable it so strongly to excite them as at once to generate the large quantity of electricity required to give a shock.

The late elaborate experiments and papers of Matteucci, though their distinct aim at first seemed to be to prove that nervous and electrical agencies were analogous, have most clearly proved that the nervous centres do not possess so much power to generate electricity as the other solids of the body, and fall far behind muscles in this respect. In his last paper, published in the Annales des Sciences Naturelles for August 1843, which is merely a continuation of those formerly published, he not only found that electrical currents were developed in a very high degree in muscles, but that these currents were totally independent of the nerves or of nervous agency, and did not even follow the course of the nerves. These experiments, therefore, are highly valuable, as tending to show that the views above entertained relative to the electrical organs generating electricity within themselves, in consequence of their peculiar structure, are probably the correct ones, seeing that electricity is developed more or less by all animal textures, and that Matteucci's experiments prove that the nervous centres generate it to a less degree than the other tissues of the body.

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* Comptes Rendus des Seances de l'Academie des Sciences, July 1841.

† Person, Sur l'hypothese des courants electrique dans les nerfs. Journal de Physiologie Experimentale, Vol. x. p. 216.

This leads us to the last and most important argument under this head, viz. have the nerves ever been shown to be conductors of electricity? Till Professor Bischoff of Heidelberg published the result of his experiments in 1841, almost every so-called fact which seemed to prove that nerves were conductors of electricity, was in reality a mere deduction from certain appearances which had been observed when electricity was applied to a nerve. That accurate experimenter, however, after a most careful investigation, found that the most delicate instruments could not only not detect any electrical currents in nerves, but that nerves were actually "very bad conductors of electricity." He, at the same time, however, ascertained that these organs manifested a very great sensibility for that agent.

M. Matteucci in his first papers endeavoured, by a reference to many experiments he performed, to prove that nerves were conductors of electricity, but a more lengthened inquiry into the subject, extended over several years, during which he performed the same or analogous experiments on the other tissues of the body, most fully satisfied him that "nerves are very feeble conductors of electricity." This change of opinion in such an able physiologist as Matteucci was the result of repeated and varied experiments on all the animal tissues. In his first experiments on the nerves and nervous centres, from finding that when several were collected together, and so placed as to form a galvanic pile, they elicited a certain amount of electricity, he was inclined to regard the nervous centres as electrical apparatus, which developed electricity or something analogous to it, and the nerves as the conductors to the extremities of that electricity which the centres generated. His subsequent experiments proved that all the other animal tissues, when formed into similar galvanic piles, not only developed a larger amount of electricity than the nervous centres, but were much superior conductors of electricity. It is surprising that physiologists should have so long overlooked this last fact—a fact which, to those who have ever experimented on the living fibre, could scarcely have been overlooked. Besides, does not the daily practice of the physician inform us, that when electricity is directed to any part of the living body it *never* takes the course of the nerves, which it would do if they were superior conductors to the other tissues of the body, but invariably crosses the whole tissues in a straight line from the positive to the negative pole of the battery? In fact, any one can very easily satisfy himself as to the low conducting power of nerve as compared with that of muscle, (for instance,) by directing a certain power of electricity along a fibre of each of equal size, and, by means of a delicate electrometer, testing the length to which the one developes a certain power of electricity as compared with that of the other. This easy means of testing the conducting power of nerve cannot fail to satisfy the most captious mind that nerves are very bad conductors of electricity, much worse, in fact, than any other of the solid tissues of the animal frame.

From all the above arguments and facts, therefore, the only conclusion which can be drawn is, that *nerves are bad conductors of electricity, and that whatever the nervous agency may be, it cannot be any thing similar to electricity or galvanism.*

But this conclusion is more than confirmed when we look to the structure and composition of the nerve itself. We all know that oil is a non-conductor of electricity; and as it must be the oil in the interior of the nervous tube, and not the coats of that tube (which do not differ in composition or conducting power from the other textures of the body) which conducts the nervous agency, common sense teaches us that it can neither be electricity nor galvanism, nor any agent similar or analogous to these, which is the agent of the transmission of nervous influence. M. Person, indeed, proved satisfactorily that the contents of the nervous tubes are the agents in transmitting the nervous influence. When he destroyed the *pulp*, as he calls it, of the nerves, but left the neurilema uninjured, he found that the nervous agency could not be transmitted past that point; and the same happened when the nerve was tied. He found, however, that the membranous tubes or neurilema from which the pulp had been expressed transmitted electricity and galvanism as freely as other membranous parts of the body. In fact he found the neurilema such a good conductor of electricity, that it freely gave off any electricity passed along it to the neighbouring parts, provided these parts lay in the direct line between the positive and negative wires used in the experiment. If the pulp, however, had been destined to conduct an electrical influence or any thing analogous to it, this neurilema ought to have been a non-conductor to prevent the dissipation of the electrical agency or its abstraction by adjoining organs.

Thus it is quite apparent, from considering the structure and composition of the nerve, as well as the facts which have been recorded relative to its power of conducting electrical agency, that the nervous agency can neither be electrical nor galvanic, nor any agent analogous to them.

Other considerations, irrespective of experiment, lead to the same conclusion. How, it may be asked, could a touch on the surface of the skin excite galvanic or electrical agency? If sensation consisted in the propagation along the nerve of a current of fluid similar to the galvanic or electric, it would be requisite that the body which produced the sensation, and excited this supposod electric current, should be in an opposite electrical state from that of the surface touched, else no electricity would be generated. This, however, we find not to be the case. Conductors and non-conductors, bodies charged both positively and negatively with electricity, bodies which give out resinous or vitreous electricity, equally act in producing sensation, and quite irrespective of the amount or kind of electricity with which they may be charged. Besides, how could the idea of an electric fluid explain the different sensations caused by different substances? How could it enable us to distinguish between a hard and a soft, between a hot and a cold body? To produce all these effects, the electric theory would require that a different kind of electricity should be produced in every case. A difference of degree would not produce the effect, for in that case a lighter touch by one substance, (say of iron,) and a heavier touch of another, (say wood,) would produce the same sensation,—which we know they do not. Besides, this theory would never enable us to account for the perception at almost the same moment of the hardness, smoothness, and temperature of a body,—all which we know we can ascertain almost at the same moment. Then, again, how could the electrical theory explain the loss of sensation and motion by a blow on the head, or by effusion of blood or fluid on the surface or within the substance of the brain? How could it explain the loss of sensation and motion of all the parts supplied by nerves below the point at which organic alteration, effusion, or compression was exerted on any part of the spinal column? How could chilling or freezing a limb induce loss of sensation or voluntary motion? How could compression of a nerve prevent the transmission of the assumed electrical agency? It is quite obvious that the electrical theory could never answer any of these postulates; and yet until it do all this it would be impossible for it to be considered as in any degree approaching the solution of the much contested question.

Had Nature intended nerves to be conductors of galvanism or electricity, or of any agent analogous to them, she would not have filled the nervous tubes with an oily fluid,—the very worst possible conductor of that subtile agent. She, on the other hand, would have filled them with an albuminous fluid, which we all know is a tolerable conductor of electricity, though inferior to the membranes themselves. So that even had experiment not demonstrated the fact that nerves are very bad conductors of electricity, the simple knowledge of their composition would have sufficed to satisfy every rational mind, that no subtile agent, similar to the electric, could be the active agent of the nervous influence.

What, then, is the nervous agency? To enable us to arrive at the solution of this question we have only to consider a few of the circumstances recorded by all experimentalists, and logically deduce a conclusion from them. It has been most satisfactorily proved, that if a nerve which goes to a muscle be cut across that nerve, on its cut extremity being irritated, whether by mechanical or electrical agency, will excite the muscle to contract. But experiment has also demonstrated that this capability of the nerve conveying irritation to the muscle rapidly diminishes after it is cut across, so that by the fourth day it is completely lost, though the muscle retains its inherent irritability for weeks thereafter. Now the simple question to answer is, what could have been conveyed along a membranous tube full of oil, which both mechanical and electrical agency could equally produce, and which the nerve, shortly after being cut across, loses the power of conveying?

The only thing which mechanical irritation could produce on a tube full of fluid is an *impulse*, *vibration*, or *wave* in the contained fluid; and all must agree that electricity could do no more to tubes full of oil. As nerves consist of membranous tubes of equal diameter, full of oil, the slightest impulse or vibration communicated to one extremity of the tube would be instantly propagated through the whole length of that tube,—and this too with the rapidity of thought, in consequence of the equal diameter of the tube, the fluid nature of its contents, and its fulness.

Let us inquire, then, how this theory, which, so far as I know, has never before been started, agrees with the actual phenomena of nature.

First, let us consider the effect of chilling or freezing a limb in producing more or less complete loss of motion and of sensation. It is known to every one, that when any of the extremities become chilled or frozen, the feelings or sensations of the part are almost or quite obliterated. This is so much the case that the part may be pretty severely handled, or even wounded, without the person being aware of having received any special injury. In fact, in a frozen limb sensation is quite destroyed. The same remarks are applicable to the power of volition or voluntary motion over a limb in this state. Just in proportion to the extent of the cooling or chilling of the limb is the loss of the power of moving it; and even before it is frozen, the power of voluntary motion as well as of sensation is completely lost. Now how does this happen? The very circumstance of cold stopping the communication of the nervous currents from the extremities of the nerves to the brain, (sensations,) and the currents from the brain towards the extremities of the nerves, (volition,) proves that it is not an electric current or anything analogous thereto, for electricity is nearly as transmissible through cold as through hot bodies, and the loss of a few degrees of temperature could never arrest the transmission of the electrical agency, and convert a nerve from a conducting to a non-conducting body. The knowledge of the structure and composition of the nerves furnishes an easy, satisfactory, and rational explanation of the phenomena. Cold freezes the oil contained within the nervous tubes. According to the degree of cold the oil is rendered more or less thick and granular. Any impulse, therefore, which is communicated to one portion is abated in intensity, or altogether lost, before it reaches the common seat of sensation; and any impulse or wave of volition, conveyed from the brain to the limb, ceases to exert its usual action, because the fluid of the nervous tube has been rendered nearly solid, and cannot receive or convey the impulse, vibration, or wave. By this simple and apparently rational view of the subject, we explain satisfactorily why the sensations and voluntary motions are blunted just in proportion to the degree of cold applied. The greater the cold the greater is the loss of sensation and of voluntary motion; and this because in proportion to the degree of cold the nervous oil is rendered less and less fluid, and obstructs more and more the transmission of the impulse, vibration, or wave.

It may be objected, that the power of voluntary motion remains in a limb long after sensation is nearly or quite destroyed; and that, before even sensation is destroyed, the temperature of the limb, as indicated by the thermometer, is much below what is required to produce congelation of the oil in the nervous tubes. It is to be recollected, however, that so long as life remains in a limb it is continually receiving a fresh supply of warm arterial blood; and as the nerves concerned in volition (those connected with the muscles) are more deeply seated than those of sensation, the power of motion must remain for a considerable while after sensation is destroyed. The supply, too, of warm blood which the parts covered by the skin are continually receiving keeps their temperature much higher than that of the surface, and prevents them from being cooled down to anything like that extent which the thermometer applied to the surface would seem to indicate. The consideration, then, of the phenomena attending the chilling or freezing a limb, seems to prove satisfactorily that it is a wave, impulse, or vibration which is transmitted through the fluid filling the nervous tubes, and not any invisible fluid or agency similar to the galvanic or electric.

Secondly, let us consider the phenomena attending that gradual but temporary compression of a nerve, which, when suddenly removed, produces that peculiar sensation known to all as the foot, hand, or limb *sleeping*. An individual sits in a certain position for some time; he is not aware of any inconvenience at the moment; but when, after a while, he moves his position, he finds that he has temporarily lost the power over one of his extremities. If the posture has been continued for any length of time, it often happens that sensation in the limb is nearly or quite destroyed. If a limb in this paralyzed state be struck or made to touch the ground, a peculiar indescribable thrilling sensation is produced as if a thousand cords were vibrating within it, giving a painfully dead, pricking sensation. How, then, do these phenomena accord with the explanation now given, of the nervous agency being a transmited impulse, vibration, or wave?

The peculiar position of the limb, whether it were the arm or leg, produced a compression of the nervous trunk. The first effect of this would be gradually to expel the fluid oil from the tubes at the point of compression. As this would occur slowly, from the weight of the limb acting on the nerve, no uneasiness would be experienced, and as no impulse, wave, or vibration, indicative of injury to the nerve, would be produced, the brain would remain unconscious of the pressure. When the limb, however, was wished to be moved, the wave or impulse of volition, conveyed from the nervous centres to the limb, would be arrested at that portion of the nerve where the pressure had removed the greater portion of the fluid capable of communicating the impulse. The distant parts, therefore, receive no impulse,—they do not execute the motions of the will,—they remain as dead parts unconscious of the volition. The limb, however, is moved by the muscles which are supplied by nerves above the point of pressure. This movement instantly removing the compressing force, allows the oil filling the nervous tubes to return to the point whence it had been expelled; the consequence of which is, that the whole fluid of the tubes is set into vibrations, and causes a peculiar thrilling sensation in the limb, a thrill which is felt extending in some cases a great way along the course of the nerve. These thrilling sensations are produced by the fluid both from above and below the point of compression flowing towards the parts which had been compressed, and which, by the removal of the pressure, is rendered in a manner a vacuum. The two streams meeting at this point are thrown into undulations or vibrations, which last for a longer or shorter time. Hence the peculiar vermicular or thrilling sensation experienced in the limb—a sensation which only abates when the power of sensation and of voluntary motion is completely restored,—in other words, when the nervous oil has come to rest. Now, it is a remarkable fact, that if a foot, for instance, in this sleeping state, be made to pat the ground, each touch distinctly renews the painful thrills, evidently from exciting new vibrations in the fluid of the nerves, and throwing it into more violent vibrations, before it has come to rest.

It seems to me quite unnecessary to show the impossibility of any supposed electrical fluid or agency giving rise to these phenomena. I have yet to learn that a pressure so trivial as that which arrests sensation and voluntary motion could prevent the transmission of an electrical current; and I have yet to learn that relieving a conducting organ from compression, (for the supporters of the electric theory only allow nerves to be conducting agents, and not the electrical apparatus itself,) would cause a development of electricity so great as would be necessary to account both for the continuance and the peculiarities of the above phenomena. The phenomena above described are solely explicable on the supposition of its being a wave, impulse, or vibration, which is transmitted through the nervous tubes, and is the cause of sensation and of voluntary motion.

The same conclusions are arrived at from the consideration of the phenomena attendant on another circumstance of every-day's occurrence. I allude to striking the elbow on any hard substance. The phenomena in this case depend upon the ulnar nerve being struck; and every one knows the peculiar painful tingling and thrilling feeling, with complete loss of power, experienced not only in the part struck, but down to the tip of the little-finger, external half of the ring-finger, and that part of the hand supplied by branches of the ulnar nerve. A distinct shock, however, if the blow be smart, is often felt striking up, as it seems, to the jaw or side of the neck. This accident is the exact counterpart of that vulgarly termed sleeping of the limb. In that, the thrilling sensation was not primarily produced by the compression, but by the sudden return of the fluid to the vacuum, formed by the removal of the pressure from the tube, thus throwing the fluid into violent vibrations. In this, again, the vibrations are primarily produced by the sudden stroke driving the fluid suddenly into waves, which run upwards towards the nervous centres, and downwards towards the extremities of the nerves. The vibrations, however, are kept up in both cases by the same cause, viz. the meeting of the return currents at the point where the vacuum was produced; and it is not till the waves or vibrations of the fluid have ceased, that all unpleasant thrilling sensations cease, or sensation and motion perfectly return.

These two phenomena, then, the exact counterparts of each other, singularly illustrate each other, and are extremely valuable, as showing how a simple theory can explain them. No electrical theory could ever explain them. No electrical theory could explain why a slow compression of a supposed conductor caused no sensation in one case, but a sudden compression of the same supposed conductor produced acute pain in the other. No electrical theory could explain why sudden compression in one case, and sudden removal of compression in another, should produce phenomena scarcely to be distinguished from each other.

The examination of the phenomena attending diseases of the nervous system infallibly lead to the same conclusions as those above arrived at. Many cases are related, a few I have witnessed myself, where a person, from having his skull driven in or depressed, has lain for days or weeks in a state of unconsciousness, the powers of sensation and of voluntary motion being almost or quite obliterated. On raising the depressed bone, however, the person awoke as from a sleep, consciousness returned, and with it the powers of sensation and of voluntary motion. The depressed bone in these cases, acting like the pressure on the nerve of the sleeping limb, prevented the free and proper transmission of the vibrations or waves through the fluid of the nervous tubes; but the moment the pressure was removed the vibrations were transmitted freely, and consciousness, with voluntary motion and sensation, were restored. The difference of effect produced by a slow and a rapid effusion of blood or fluid on the surface of, or within the brain, receive an equally satisfactory explanation by the above theory. When the effusion is slow, the parts are gradually accommodated to the increased pressure; the powers of sensation and of voluntary motion therefore remain. But if the effusion be sudden, the nervous tubes are put in the same state as where a nerve is pressed, the vibrations communicated to the nervous fluid cannot be communicated through the tubes, so that the individual loses the powers of sensation and of voluntary motion. But just in proportion as the parts are relieved from compression by natural or artificial means, provided structural changes have not been superinduced, the functions of the brain and nerves are restored.

The phenomena observed in diseases of the spinal cord from alterations of its natural structure, or from compression produced by effusion of blood or dislocated vertebræ, equally illustrate the theory that it is a wave or vibration, and not an electrical or analogous fluid, which is transmitted through the nervous tubes. How could compression of any portion of the spinal marrow prevent the transmission of a subtile electric fluid? How could softening, or the very opposite state, hardening of the cord, prevent this theoretic electric fluid being transmitted to or from the *sensorium commune?* Neither softening, nor hardening, nor compression of the spinal marrow would prevent volition being conveyed from the brain to distant parts, nor sensations from distant parts from being transmitted to the brain, were the agent electrical, or anything approaching thereto. It is well known, however, that such pathological states do most certainly deprive both of motion and of sensation the parts supplied by nerves beyond the point diseased; and the only possible way of explaining this fact is that which I have endeavoured to explain—viz. that it is a wave, impulse, or vibration which is conveyed through the fluid of the nervous tubes. These three pathological states of the spinal cord would act so far similarly that they would all arrest the progressive wave or vibration of the fluid in the nervous tubes; the pressure, by directly compressing or emptying the tubes,—the other two, by the morbid changes having so altered the structure of the tubes, or of their contents, as to unfit them for performing their functions.

The following facts seem also fully to corroborate the supposition that it is a wave, impulse, or vibration, and not electrical fluid, which is conveyed through the nervous tubes. When a person has his body immersed in water, as in bathing, but little pain or uneasiness is felt from injuring the surface, in consequence of coming in contact with rocks, logs of wood, &c. so long as the part so injured is kept under water. This is a fact familiar to every bather, and one as to whose truth I can vouch from repeated personal experience. Now, what is it which prevents the nerves from transmitting to the brain the full knowledge of the injury? If sensation depended on the transmission of an electric current along the nerve, how could immersion of the body in water prevent that transmission from being effected? How could the surrounding of the limb with water prevent the supposed generation of electricity at the part wounded? Both these questions are unanswerable according to the electric theory, yet receive a ready and intelligible explanation according to the views which I have been led to adopt.

Immersion of the body in the denser medium of water must blunt sensation just in proportion as the density of that fluid exceeds that which naturally surrounds our bodies. The pressure of the denser medium renders the waves or vibrations of the fluid in the nervous tubes much less intense, so that sensations which would be felt acutely in the air, are only very bluntly perceived when the body is immersed in sea water, even though the part be in addition irritated by contact with the briny fluid. The denser, therefore, the medium, the less acute, or the blunter, will be the sensations. After arriving at these conclusions, I made several experiments for the purpose of ascertaining approximatively if such were really the case, and I found them to accord strictly with the fact. Thus I found that when the body was immersed in fresh water, the surface of the body could be scratched, cut, or otherwise injured without conveying to the *sensorium commune* any knowledge of the injury, at all commensurate with what would be felt were the same done in the air; in fact, when the surface was injured accidentally, it often happened that I was unaware of it till I rose from the water. The sensations, however, were in every case much more blunted when the body was immersed in sea-water; indeed in many instances in which I had scratched and cut myself rather severely, (but accidentally) on rocks, I was not aware of having sustained the slightest injury till I rose from the water, when the intense smarting soon made me recognise the nature and extent of the injury, and also served strongly to direct my attention to the difference of the sensations experienced in the air, from those felt under the water. When under the water I had merely been aware that the part of my body which was found injured had come in contact with rocks or other hard bodies.

Immersion of the hand in a jar of mercury, a denser fluid still, seemed to deprive it of sensation altogether; at least it felt as if dead, or as if it had lost the power of sensation from being frozen.

Although these phenomena, which possess the additional advantage of being known to every one, seem to prove satisfactorily both the fallacy of the electric theory, and the truth of the vibration or wave theory, it appears to me that there is one other illustration which may be adduced, which of itself, though the rest were insufficient for the purpose, would prove the correctness of my theory. This is *the mode by which the nerves of the ear receive impressions of sound.*

No one will be foolhardy enough to assert that it is electricity which is the cause of sound being communicated to the ear. The principles of the conduction of sound are so well established, that it is admitted by every one that it is by vibrations alone that sound is communicated to the ear. According to the modern theory of the nervous agency, these vibrations must excite an electric current in the nervous fibrils of the ear, in order to convey the sensation of their vibrations to the brain. I am not aware that any writer has viewed the subject in this light, for they all appear to have left off the inquiry as soon as they brought the vibrations of sound to the nerves of the ear; but if their theory be applicable to one nerve, it must be applicable to all, and that theory requires such a postulate—requires that external impressions and vibrations of sound should generate an electrical current in the nerve, in order to convey the sensation or perception of sounds to the brain. All, I think, will be agreed that it is unnecessary to attempt to prove that vibrations of air could not generate an electrical current. How, indeed, could an electrical current take cognizance of the minute combinations and varieties of sound (or intensity of vibrations,) which it would require to do were it the agent by which the vibrations produced by an orchestral performance would require to be transmitted to the brain?

Let us attend for an instant to the beauty and perfection of the apparatus destined for the conduction of the sensations of sound to the brain. First, we have an organ (external ear,) which collects the vibrations of sound, and by its particular arrangement conducts them to a membrane (of the tympanum,) whose state of tension can be increased or diminished by the more than half involuntary muscles attached to the chain of bones in connection with it. To the back of this membrane is attached a bent chain of bones whose other extremity is fixed to a membrane (of the *fenestra ovalis*,) which covers the principal aperture of the bony cavity which constitutes the internal ear, in which the fine nerves of the ear are spread. This bony cavity, variously chambered and contorted, is filled with a fluid, and in this fluid the nerves of hearing lie loose and expanded. The vibrations of sound collected by the external ear, and conducted to the tympanal membrane, communicate their vibrations to it. Every vibration of this membrane is at once communicated through the chain of bones to the membrane which covers the cavity of the internal ear; and as this membrane is in contact with fluid in which float the nerves of hearing, the vibrations are conveyed as vibrations to the nerves themselves. *As these nerves consist of tubes filled with a fluid, the vibrations communicated to the fluid in which they float are transmitted as vibrations to the fluid within their tubes, so that the vibrations of sound are communicated as vibrations to the brain itself.* Whatever, then, be the intensity of the vibrations, the same is communicated to the nervous fluid, and thence to the *sensorium commune*. In fact, this is the only way in which it is possible for the brain to distinguish between different sounds.

If this, then, be a case in which it can be distinctly proved that the agent by which sensations are communicated to the nervous centre is a wave or vibration, it is only following the plain dictates of reason to infer that sensation of all kinds must be communicated in the very same manner. As all the nerves are of the same structure, as all are composed of tubes filled with oil, as all are non-conductors of electricity, the only inference which can be drawn is, *that the means by which sensation is con-*

veyed to the nervous centres is by means of vibrations, or waves transmitted through the fluid filling the nervous tubes.

If we enter a little more largely into the consideration of the different kinds of sensation, and endeavour to ascertain how the explanation just given accords with what we know of the effects of the application of different external agents, we shall feel the more assured that this theory is probably the correct one.

Let us therefore inquire into the phenomena attending the application of heat and of cold. As a general rule, heat exalts the sensations,—cold depresses or blunts them. Hence, among the cold-blooded animals whose temperature is pretty much regulated by that of the surrounding medium, we find that all are most active during the summer and heat of the day, but more or less torpid or sluggish during winter and the cool of night. The heat in this case acts by rendering more fluid the oil contained within the nervous tubes.—Hence the greater aptitude to receive impulses, and a more perfect transmission of these to the great nervous centres. Cold, on the other hand, acts by producing a greater or less loss of fluidity in the oily contents of the nervous tubes.—Hence a diminished susceptibility to receive impressions, and a more imperfect transmission of the impulses or vibrations.

When a cold substance is applied to the surface of the body, a sensation is experienced which practice enables us to recognise as produced by something cold. How, it may be asked, does a cold body, or simply cold, act in producing a wave or vibration in the oily fluid of the nervous tubes? Of all the animal fluids, oil is that which exhibits the greatest amount of dilatation under heat, and of contraction under exposure to cold. When a cold body, therefore, is applied to the surface of the body, the oil filling the nervous tubes at the point of contact with that body is chilled, consequently contracts, and thus produces a flow of the fluid in the vicinity towards this part,—a flow which necessarily produces a vibration, and which, being transmitted to the common centre of perception, produces that sensation which custom has taught us is caused by a cold body.

The phenomena, however, admit of another explanation, yet equally favourable to the vibration theory. The cold may act *primarily* on the membranous nervous tubes, in the same manner as we see it does on all the other tissues of the body. It would thus cause them to contract on their fluid contents, and thus equally give rise to a vibration or wave.

The following fact may be regarded as proving that cold produces sensation in some such way as that above described. It is familiar to all, that if a cold body be touched, a perceptible, though short, interval elapses, after we are aware we have touched the body before we perceive its actual temperature. This shows that the cold must have time to act specifically on the tissues concerned in the transmission of sensation before it gives rise to that form of impulse or vibration which conveys to our minds the impression of cold.

The converse of all this is true of a warm body, or of simple heat. If we touch a warm or hot body, the oil in the nervous tubes is dilated; a reflex wave or vibration is produced in the fluid nervous oil, and hence there is transmitted to the nervous centres that perception which custom has taught us is produced by a hot body. The sensation of cold would therefore seem to be produced by a wave flowing *towards* the point of contact with the cold body—the sensation of heat by a wave flowing *from* the point of contact with the hot body. If, however, the heat and cold acted primarily on the membranous tubes, the converse of this would be true; but in either view the primary direction of the vibrations would be opposite when heat and cold were applied, and this is probably the reason why we distinguish them so easily from one another.

It is worthy of remark that the sensation experienced by touching a very hot body and a very cold one are extremely similar—so similar, indeed, that the sensation produced by touching a piece of frozen mercury and a hot bar of iron cannot be distinguished. This is a fact which no electrical theory could ever account for, but receives an intelligible explanation by the vibration theory. The sudden heat would not only cause the tissues to become immediately overloaded with blood and thus compress the nerves, but the oil in the nervous tubes would be so much expanded as to produce a reflex wave or vibration. The sudden cold, by constringing all the tissues, would also cause them to compress the nerves, while the membranous tubes, by contracting over their oily contents, would also primarily cause a reflex wave or vibration to be produced.

A still more important fact, however, to attend to, is, that if the contact with the freezing body be prolonged for a certain time, the sensation of the part is quite extinguished. Any one may satisfy himself of this by immersing the hand or finger in a freezing mixture. According to the theory just advocated, this phenomena receives a ready explanation. The oil in the nervous tubes becomes more or less consolidated, so that it is no longer capable of transmitting any vibration or wave. Sensation is therefore temporarily suspended. M. Longet, in his recent work on the nervous system, mentions a fact corroborative of the above conclusions. When he applied ice to a nerve, for a very short time, he found it lost its excitability so completely that no irritation of it by mechanical or galvanic agencies could excite to contraction the muscles to which it was distributed.

Did we know anything relative to the causes of the difference in the tastes or odours of bodies, some remarks might be made on the applicability of the above theory to the sensations of taste and smell; but as we are at present completely in the dark as to the physiological reason why one body gives a saline, another a saccharine, one a sapid, another an insipid taste; and why one body has an agreeable, another a disgusting odour; it may seem little better than waste of time to state my views of the matter. As the slight attention, however, I have given to the subject may tend to induce others, more competent to the task, to take up the inquiry, I shall state very shortly the imperfect results at which I have arrived.

In attending to the anatomical relations of the nerves of taste, we find that the extreme meshes of the gustatory nerves are chiefly collected in masses, and assist in forming those small vascular organs termed the papillæ of the tongue. When we desire to taste any body, a greater quantity of blood seems to be sent to these organs, (which in this respect resemble the erectile tissue,) the nervous energy appears in consequence to become exalted, and when the sapid body reaches these organs a peculiar sensation is experienced, which we denominate the taste of the body. Many, perhaps all, bodies capable of being tasted, seem to act chemically on the nervous and vascular papillæ; that is to say, they exert the same kind of action on them as they would do were they applied to any membrane or tissue of the body. Thus we know that if a sapid body, as for instance a salt, be laid on the tongue, no taste is experienced unless some portion of it be dissolved. We know that saline solutions harden or contract animal substances, but in different degrees, according to the nature of the salt, and the varying affinity of its constituents for those of the animal tissue. We have only to suppose, what in reality takes place, that the saline solution acts in a similar manner on the papillæ of the tongue, viz. produces either their dilatation or contraction, and we have a cause sufficient to give rise to a wave or vibration of the fluid contained in the nervous tubes of these papillæ. These vibrations being transmitted to that particular part of the brain destined to receive the impressions from the organs of taste, cause us to recognise the nature of the sapid body. Every sapid body probably acts in a somewhat analogous manner; for it seems to be a law of nature that no substance can be tasted unless some portion of it, often a very minute one, be dissolved, and be brought in contact with the papillæ.

There appears to be no greater difficulty in explaining the very various tastes of bodies, or how each is transmitted individually to the sensorium, than there is of accounting for the diversities of sounds or of the ordinary sensations. All would seem to be resolvable into the varying intensity and nature of the vibrations or undulations into which the fluid of the nervous tubes is thrown; and, in fact, it is now satisfactorily ascertained, that it is the very same nerves which preside over the common sensations of the tongue and palate, and the special sense of taste, viz. the lingual branch of the trifacial and the glosso-pharyngeal nerves.

Of sapid bodies, the action of none is perhaps so easy to trace as that of the class of astringents. By the application of this class of bodies to the tongue the papillæ are distinctly contracted; and as the terminal loops of the nerves form an integral portion of these bodies, the membranous nervous tubes are contracted at the same time, and produce in their fluid contents undulations or vibrations which convey the sense of astringency to the sensorium. If, then, it can thus be shown to be probable that vibrations could thus be excited in the fluid of the gustatory nerves, it is reasonable to infer that in every other case they are similarly acted on, although, from our imperfect knowledge of the action of these sapid bodies, we may not be able to show how this is effected.

It is highly probable that the nerves of smell are acted on in precisely the same way as those of taste. We know that for the perfection of the sense of smell it is requisite that the nostril be moist; and we also know that this sense is destroyed or greatly impaired by preternatural dryness of the nostril as well as by an unusual mucous discharge. This seems to indicate that it is necessary the odour should be arrested or fixed by the natural moisture of the nostril, and, though invisible to us and eluding our grasp, be applied as a substance to the terminal loops of those nerves which are specially destined for the sense of smell. Hence, if the nostril be too dry, the odour passes over it without becoming fixed by the moisture, so that little or none is applied to the nerves and no smell is perceived. If the discharge of moisture or of mucus be excessive, or of too gluey a nature, the sense of smell is rendered equally imperfect, because the odorous particles are too much diluted, or are carried off before they have had time to act on the nerves of smell, or the thick discharge altogether prevents their reaching the nerves.

Now, so far as the nature of odorous substances has been ascertained, it seems to be proved that they are always material, even though the infinitely small quantity of matter diffused through the air, and recognisable by the sense of smell, be inappreciable to all our other senses, or to the most delicate analysis. Thus most of the odorous parts of plants exist as volatile oils or resins; and those of animals, (castor, ambergris, musk, &c.,) bear a close analogy to them. It is only when portions of these are dissolved in the air and brought in contact with the olfactory nerves, by being abstracted from the air, and fixed by the natural moisture of the nostril, that the sense of smell is exercised. It is extremely probable that the odorous vapour thus condensed by the moisture of the nostril acts in precisely the same way on the delicate olfactory nerve, as the sapid body did on the nerves of taste,—acts by producing a vibration or undulation of the oily fluid of the nervous tubes, which, according to its intensity and nature, communicates to the brain the perception of the odour applied.

Though I have not yet paid that attention to the sense of vision which its importance demands, I cannot for one moment doubt that it is also by vibrations or undulations of the fluid in the nervous tubes of the optic nerve that the perception of light and of external objects is conveyed to the brain. The latest researches show that light must be considered as a material substance; and of the two theories which at present agitate the philosophic world, that which seems to me to accord best with the latest researches, is the one defended and illustrated by Descartes, Huygens, and Euler, that "all the phenomena of light depend on the undulations of a highly attenuated fluid or ether, which, when at rest, is inappreciable to our senses; but when acted on by luminous bodies is thrown into undulations or waves." If this theory wanted additional proofs of its correctness, it appears to me that it would receive a very strong one in the case before us. All fluids move in waves, undulations, or vibrations; and when we find that the structure of the optic nerves is the same as those of other parts of the body,—that they are all composed of membranous tubes filled with a fluid oil,—when it can be distinctly proved in several cases that perceptions or sensations are conveyed to the sensorium in consequence of vibrations or undulations being communicated to the fluid in these tubes,—the only natural conclusion at which it seems possible to arrive is, that in every case it is by means of vibrations or undulations excited in the nervous fluid that the perception of external agencies is conveyed to the brain.

The nerves of vision, therefore, can form no exception to the general rule. The fluid filling their nervous tubes must be thrown into vibrations or undulations in an analogous manner to that filling the tubes of the nerves of hearing, viz. by the communication to it of the undulations or vibrations of that fluid which is the cause of the production of light. Seeing that this is the case, these illustrious philosophers above named did well to compare the undulations of light to those which sonorous bodies produce on air; and this theory, while it explains and illustrates the mode by which sensations of light, and of course the visible appearance of all bodies, is conveyed through the nerves of the eye to the brain, is itself powerfully supported by the above investigations into the structure of the nerves, and into the mode by which sensations of all kinds are conveyed to the *sensorium commune.*

Seeing, then, that the above inquiry leads to the conclusion that sensation is in every case communicated to the nervous centres by means of vibrations or undulations excited in the fluid of the nervous tubes, it seems equally fair to infer that the same means are employed to convey volition, or the exciting power of voluntary and involuntary motion from the nervous centres to the other parts of the body. The structure of the nerves of motion does not differ from those of sensation. External agencies give rise to the vibrations or undulations in the one case—internal impulses give origin to the other. It is no argument against this theory that we cannot demonstrate how the impulses of volition could originate in the brain or other nervous centres. Such a power must reside in the living principle. But as we know absolutely nothing of the connection between mind and matter, it is useless to attempt an explanation of this part of the subject.

As it may be objected that the idea of vibrations being the means by which sensation and volition are effected would not account for the instantaneous conduction of the impression, I made the following experiment. No nerves are three feet long before they reach a nervous centre; a fine glass tube, upwards of three feet long, was therefore bent up at one extremity for about one and a half inch, and at the other for about three inches. The shorter extremity was covered with soft bladder and the whole tube filled with oil, care being taken that no air was left between the bladder and oil. The longer extremity was then filled to within half an inch of the aperture, and a float with a central upright wire placed on the surface of the oil. The upright wire pressed against the shorter limb of a finely balanced index whose longer limb moved circularly over an upright circular marked card. The whole was securely fixed to an immovable table. Whenever the soft bladder was touched so as to press it against the oily fluid in the tube, that instant the movement of the long index over the card showed the instantaneous conduction of the vibration through the fluid. The conduction was so instantaneous that the index moved at the very same moment I was myself sensible I had touched the distended bladder covering the short extremity of the tube.

The subject of the nerves of involuntary motion, and the mode by which the vibrations or undulations are excited in their nervous fluid, is reserved for a future paper. Suffice it to state at present that they also act by the constant excitement of vibrations in their nervous fluid.

As connected with the subject of this paper, there is one subject which has long proved a stumbling-block to the philosophic inquirer, which seems to receive a satisfactory explanation from these researches—I mean the hybernation of animals. No writer with whose works I am acquainted gives to my mind even a rational explanation of the cause of this most curious phenomenon; but it appears to me that the discovery of the true structure and composition of the nerves affords a complete solution of the problem.

All the cold-blooded animals which live on the land in cold or temperate regions, and a few of the warm-blooded, pass the winter in a state of torpor or of sleep—*hybernate* as it is termed. The cold acting on their nerves, as it has been shown above it acts on the nerves of a limb which is chilled or frozen, consolidates more or less the oily fluid, and consequently unfits the nerves for the performance of the functions of active life. The nervous system reacting on the circulating system, causes increased slowness of the pulsations of the heart, so that the blood is rendered nearly but not quite stagnant. The slow motion of the blood and the torpor of the nervous system naturally lead to increased slowness of the respiration, the consequence of which is, that the animal's temperature cools down so as to be only a few degrees above the freezing point.

When heat is applied to an animal in this torpid state we see by slow degrees its chilled limbs relax and become more pliant; its respirations become more frequent; its temperature rises; voluntary motion and sensation slowly return; and in a short while the functions of active life are resumed.

How beautifully does the above theory explain all these phenomena. The increased temperature renders more fluid the oil contained in the nervous tubes; sensibility, which was previously almost gone, is thus increased. The necessity for respiration being then more strongly felt, the chest is more fully dilated; the blood thus flows more freely to the heart; the heart's action is consequently quickened, and the increased stimulus thus given to the whole system causes life in all its vigour to be once more manifested.

It has been long noticed that hybernating animals become very fat before going to their long winter's sleep, but come out in spring reduced to perfect skeletons. Mr Grisenthwaite* was the first to show that the lungs were a furnace, for the purpose of keeping up the animal heat, and that the use of the food was to supply fuel to that furnace. He was also the first to show that the thick layer of fat in the hybernating animals was to supply carbon to the lungs during their long winter sleep, and thus keep the animal temperature from falling below that point necessary to sustain life. In my own researches on this subject, I ascertained that all those animals, mammiferous as well as reptiles, which spent the winter in dry burrows, laid on this fatty layer previous to retiring to their winter's sleep. *But that all those which buried themselves in the mud, and consequently did not respire, did not assume this layer of fatty matter.* This seems to be in strict accordance with the wise law of Nature of doing nothing in vain. Thus I found that hedgehogs, marmots, dormice, serpents, lizards, and other reptiles which pass the winter in dry burrows on the land, become fat before the approach of winter. This is so remarkably the case with some snakes, that I have seen a thick layer of semifluid fat around their bodies towards the end of autumn, and yet, when they first appear in spring, they are well known to be so very lean, that their vertebral ridge is distinctly seen projecting under their skin. The same is true with respect to the lizard tribes, and from having often kept them in a state of domestication, I have had ample opportunities of witnessing the fact. This circumstance would seem to show that reptiles, as well as their warmer-blooded fellow-sleepers, breathe during their long winter's sleep, and thus consume the carbon of the fat during the winter.

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* Essay on Food, &c. London, 1838.

I have never been able to detect any such fatty layer in frogs, toads, and newts—reptiles which bury themselves under the mud during the winter, and consequently do not respire all that time. Indeed, a layer of fat would have been worse than useless to these animals, seeing its greater buoyancy would have subjected them to the risk of being floated out of the mud during winter.

The theory which I have started relative to the cause of hybernation, throws no small light on a phenomenon experienced in very cold countries, and nearly proved fatal to Dr Solander, the friend of the illustrious Sir Joseph Banks. I allude to the effect of exposure to cold in producing that irresistible drowsiness, to which he who gives way sleeps to wake no more. The phenomena observed in this case are precisely similar to those witnessed in the hybernating animal. The limbs become chilled and fail; listlessness, with an irresistible torpor and tendency to sleep, comes on; nothing can induce him to move, even the knowledge that to sleep is to die; he gives way to the resistless impulse to sleep—to sleep that sleep from which there is no awaking. The circumstances attending this drowsiness, caused by exposure to cold, have been brought so repeatedly before the notice of the public, since the publication of the conquest of Peru by Pizarro, where from this cause he lost so many men, that it is unnecessary to enter into further particulars regarding it.

The effect of the cold is first to chill the extremities and surface of the body, and then render more consistent the oily fluid filling the mucous tubes; while, at the time, the blood expelled from the extremities is thrown on the brain and internal organs. The sensations are therefore blunted, and at the same time the powers of volition or voluntary motion are in the same proportion destroyed. Hence the feeling of lassitude, and both disinclination and inability to move. Hence, when roused, their declaration that there is nothing the matter with them; that they are perfectly comfortable, though, at the same time, the extremities may be actually frost-bitten. As the cold continues to act, the nervous oily fluid becomes thicker and thicker, and this rapidly extending to all parts of the body, produces that loss of nervous energy which we denominate sleep. This reacting on the circulating and respiratory systems produces increased slowness of the breathing and of the pulsations of the heart, and, as it continues, final stagnation. When this stage arrives, death has closed the scene.

Such are a few of the interesting topics on which the discovery of the true structure and composition of the nerves seems to throw light. And, in so far as the nervous agency is concerned, I have no doubt that the more attentively the subject is examined, the more clearly will it be proved that it simply consists in the oily fluid of the nervous tubes being thrown into vibrations or undulations.

21, *Heriot Row.*

Art. III.—*The Constitution and Diseases of Women. Part II. Containing the Theory and Diseases of the Catamenia.* By Charles Bell, M.D., Fellow of the Royal College of Physicians, Edinburgh, and Assistant Physician to the Edinburgh Maternity Hospital. (Continued from p. 27.)

Having considered the structure of the uterus, our next object, according to the present arrangement, is the investigation of the catamenial discharge. This is a secretion peculiar to woman, and from its great importance in the female system, and the evident influence which it exercises over her health and comfort, it has in all ages been a subject of deep interest and attention.

Although ancient authors have devoted much space in their works to the consideration of this function, it is not surprising, when we consider the very defective state of their knowledge of anatomy and physiology generally, that they should afford us little assistance in our investigation. Their descriptions are of the most superficial nature, and are in general a mere detail of the popular prejudices which prevailed at the period at which they wrote, or a repetition of the crude notions of their predecessors, into the truth of which they seem to have taken no trouble to inquire. We have in consequence the most exaggerated and inconsistent properties and uses assigned to this secretion. As, however, it is interesting to trace the progress of every branch of science, and to review the opinions which have been suggested from time to time on the subject, we shall now take a brief survey of some of the various theories which have been handed down to us by the most celebrated authors in reference to the catamenia.

From the earliest ages to a comparatively recent period this secretion was supposed to carry off noxious humours from the body;* and, in consequence, it was universally believed to be one of the most deadly poisons in nature; that it was destructive not only to animal and vegetable life, but most injurious to inanimate objects, when it came in contact with them. In example of these exaggerated notions, and of the very superficial manner in which the subject was investigated, we find the following alarming catalogue given of its properties by De Graaf, which is nearly a repetition of the opinion entertained by Pliny.—"Praeterea sanguinis illius contactu penis cum menstruata concumbentis excoriatur; si novella vitis eo tangatur in perpetuum læditur; steriles fiunt tactæ fruges, moriuntur insita, exuruntur hortorum germina; si mulier prægnans alterius menstrua supergrediatur, aut illis circumlinatur, abortum facit; ei autem quæ uterum non gestat, concipiendi spem adimit: purgantis spiritus et vapor ab ore specula atque eboris nitorem obscurat: gustatus hic sanguis canes in rabiem agit, homines vero diris cruciatibus affligit, comitialem morbum, pilorum effluvium, aliaque elephantiorum vitia infert." Opera Omnia, p. 263.

The picture given of the catamenia in this passage is so totally without foundation, and its inconsistency so evident, that it is very unaccountable how it should ever have been countenanced by intelligent men. The only grounds which can be assigned for a prejudice so contrary to the facts which are daily presented to us, are the various painful and fatal diseases which occur in those women whose catamenia are interrupted. But whilst it is difficult to account for the origin of a prejudice so void of foundation, it is not at all surprising, that, having been promulgated, it should be handed down from one generation to another among the barbarous and ignorant, because the laws and customs formed in consequence of the prevalence of such notions were so rigorously enforced and blindly followed, that no opportunity was afforded for discovering their fallacy. By those laws women were considered unclean, and strictly prohibited from holding any intercourse with their neighbours during their monthly periods; and were compelled either to seclude themselves entirely from view, or to wear some peculiar dress by which their state might be known and themselves avoided. These customs still prevail in some of the aboriginal nations in America, as well as in many of the countries within the tropics;† and among the Hindoos the women are dressed in red during their monthly periods, and strictly secluded from their families.

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* "Qui tamen nonnunquam ita vitiatur ut pene inter venena referendus sit." Freind's Emmenologia, p. 2.

† Moreau Histoire Naturelle de Femme, Vol. i. p. 518.

The laws of Moses,* whilst they prove the high antiquity of these customs, also tend to the belief that that inspired individual was impressed with the prejudices entertained with regard to the nature of the catamenia; but if the whole tenor of his ordinances be taken into consideration, it will become evident that his object was, to inculcate that purity of manners and personal cleanliness so necessary for the preservation of health in warm climates.

Although among civilized nations women are no longer subjected to those harsh and absurd restraints during the period of the catamenia, and more enlightened views are generally entertained on the subject, we find some lurking remains of the errors of the barbarous ages still existing among women themselves. Hence many are averse to perform culinary duties during menstruation, from a belief that bad effects will result to the food; and it is a prevailing opinion among all classes that meat salted by a woman at that period is unfit for use, and rapidly falls into decay.†

The cause of the catamenial discharge was as little understood by the ancients, as its nature, and their opinions in reference to it are not more rational or instructive.

The most ancient speculation on this subject attributed the flow of catamenia to the influence of the moon. This notion was supported by Aristotle, who assigned it as one reason for the moon being considered of the female sex.‡ In modern times the same idea has been maintained by Dr Mead,§ who asserts that in the regions more near the equator, the stronger is the influence of the moon on the fluids of the body, and hence that the catamenia are more profuse in women inhabiting those regions; while the contrary is the case in the countries nearer the poles. He also quotes some cases to prove that the moon occasionally produced similar discharges in men of feeble constitution.

This theory seems to have arisen partly from the apparent influence of the moon on the tides of the sea; hence it was supposed that it must have a similar effect on the fluids of the human body. But it is principally to be attributed to the apparent coincidence between the evolution of the moon and the monthly period in women. This seeming correspondence between those two events, however, is more apparent than real, as we find, on investigation, that they differ materially from each other in point of time,—the evolution of the moon requiring twenty-nine days, twelve hours, and forty minutes for its accomplishment, while the catamenial discharge, when regular, returns every twenty-eight days. Besides, if this discharge depends on the full moon, all women who are neither sick nor pregnant should menstruate at the same time,—a circumstance which daily experience must have disproved. These facts have been entirely overlooked by those who maintain that the lunar influence is the source of this extraordinary provision in the female system. It is, therefore, evident that the older writers were more anxious to hand down the errors and prejudices of their day than to correct such palpable fallacies.

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* Leviticus, chap. xv.

† La Motte has such implicit belief in this opinion, and carries his prejudice so far, as to assert that red-haired women are worse than any other in this respect. In corroboration of this assertion, he gives a ludicrous anecdote of a red-haired servant of his who spoiled some choice wine, just when he was going to sit down to enjoy it; and he assures us that the instant she touched the bottles the wine was converted into vinegar, much to the annoyance of himself and his guests.—Traité des Accouchmens, p. 57.

‡ See Aristotle, Opera Omnia, cap. 7, Latina.

§ Mead De Imperio Solis et Lunæ, p. 47.

Passing over, as either unfounded, or as little worthy of notice, the hypothesis of Galen, Boerhaave, Sylvius, De Graaf, and Freind, who assigned general plethora as the cause of menstruation, we come to the opinions of more recent authors, who, arguing on the same assumption as their predecessors, ascribe the catamenia to a local plethora of the womb itself. Haller, Cullen, and Astruc entertained this opinion. Haller supposed that the veins of this organ became periodically so much distended with blood, that their coats were ruptured. Cullen imagined that the arteries of the womb were the immediate source of the catamenial discharge, and that, when it was once established, it returned periodically from the mere force of habit. Astruc gives a more ingenious and complicated explanation of the matter. He supposes that the uterine arteries, after ramifying in innumerable small and tortuous branches between the inner and middle coats of the uterus, terminate in canals of a cylindrical form;—that these canals are the capillary origins of the veins, and that after repeated unions with each other, they form the venous trunks;—that from each of these points of union arise perpendicularly venous appendages, which penetrate the lining membrane of the uterus, and extend beyond it about four lines; that in the gravid state these appendages are the medium of attachment for the placenta, and supply that organ with blood, whilst in the unimpregnated state they discharge the menses without being ruptured; and that if performing neither of these offices, these appendages are collapsed, and invisible to the naked eye. In addition to these vessels, he considers that others of a smaller size and white colour, resembling silk thread, exist, but are invisible, like the appendages, to the naked eye, except in the advanced months of pregnancy; and that these vessels also penetrate the lining membrane, and pour a milky fluid into it for the nourishment of the fœtus. These he denominates vermicular vessels, under the supposition that they surround, and are intimately connected with the veins. He farther supposes that the thinner part of the fluid which circulates through these vessels in the unimpregnated state is absorbed, whilst the thicker part is accumulated in them, and gradually renders them turgid, when they so compress the veins, that the blood regurgitates into the venous appendages, and thence into the uterus in the form of the menses.[*]

The hypothesis that the menses depend entirely upon a local plethora, cannot be admitted, because, were this the sole cause of the discharge, we should have the catamenia flowing whenever there was an increased action in the uterus, and especially in congestion, which is not confirmed by experience. There is another most essential objection to both general and local plethora, in the fact that the menses are found to occur regularly in those recovering from long sickness and debility, a condition in which plethora is not likely to occur.

Lecat imagines that the catamenia arise from the venereal impulse producing a flow of the blood into the vessels of the uterus in the same manner as takes place in the *corpus cavernosum penis*, and that in consequence of the resistance and pressure of the neighbouring parts, the vessels give way, discharging their contents into the cavity of the uterus. This theory is more particularly deserving of notice, as it appears to have been adopted by Mr Abernethy in a lecture which he delivered before the Royal College of Surgeons of London, 1817. He expresses himself thus. "Why Nature should have doomed the human female to the periodical loss of so much nutriment, and proportionate power, is a problem that can only, in my opinion, be solved by supposing that it relieves uterine irritation, and mitigates the extreme of sexual desire, thus enabling her to conform to the laws of morality and social compact that are established between us." It is surprising that a person of Mr Abernethy's eminence and accurate observation should have expressed himself so inconsiderately, and exhibited so total an ignorance of the subject of which he was treating. It proves that he had a very vague and imperfect knowledge of the cause and object of the catamenia, and at the same time it places in a very degrading view the high moral sentiments which adorn and give worth to the female character. Not only is there no distinct evidence of any connection between the catamenial discharge and the sexual passions,[†] or that they are more easily excited immediately before or during its flow,[‡] but there is every reason to believe that there is a natural disinclination in both sexes to have sexual intercourse at that period. Farther, although in some instances there is a certain degree of lassitude during the flow of the catamenia, there is in general no decided indication of loss of power observable consequent on the discharge. On the contrary we find young women enjoying the same vigour and good health immediately after menstruation as before, and fully as anxious to enter into all the amusements of youth as at any other period. It is also observed that the body continues of the same weight.* It is worthy of remark, too, that women almost invariably date their becoming pregnant to the period shortly after having menstruated. This fact indicates that there must be some peculiarity in their sensations which induces them to fix on such a period more than any other; and as their reckoning generally proves correct, it is not unreasonable to suppose that the female system is rendered more fit for the function of gestation by the act of menstruation.

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[*] Burdach seems to have a similar notion of the immediate source of the menses, if he has not borrowed the idea from Astruc. He says, "Il parait oiseux de rechercher si le sang coule des arteres ou des veines ; car, en tout cas, ce liquide provient des ramifications les plus déliées, ou des vaisseaux capillaires, qui tiennent le milieu entre les arteres et les veines et les unissent ensemble, sans appartenir ni aux uns ni aux autres.—Burdach's Physiologie Trad. par. A. J. L. Jourdan, Tome i. p. 286.

[†] "But the menstruation of the human female is a phenomenon of a totally different nature (from heat in animals), and has no connection with sexual excitement." Müller's Phy. Trans. by Baly, p. 1482.

[‡] Pendant la menstruation il n'y a point desir de l'act vénérien par conséquent point cette exaltation de tontes les forces qui caracterise l'etat des femelles en chaleur." Burdach, Physic Traduite, par A. J. L. Jourdan, p. 300.

In more recent times we find Burdach† supporting many of the errors of the older authors; but being dissatisfied with the terms local and general plethora, he substitutes that of "la force plastique," which he considers is more energetic in women than in men, and that the quantity of the catamenia is in proportion to the intensity of this principle. This plastic power he considers acts both generally and locally, and depends on the disposition to form a greater quantity of blood loaded with an excess of carbon than the lungs are capable of purifying; or, in other words, that the power of chylification is more active than respiration,—a circumstance which he ascribes to the lungs being of a disproportionate size in the female; and that in order to relieve the system from the oppression which this state of the organization produces, a supplementary lung,—"un poumon accessoire," is required, and he assumes that this is to be found in the uterus; because, in his opinion, there is a strong analogy between the structure of that organ and that of the lungs. The uterus, he asserts, effects that change in the blood which the lungs have left incomplete, and it accomplishes this by means of the catamenial discharge, which contains a large proportion of carbon. The uterus, he thinks, is enabled to perform this function by means of its pendant situation, the greater size of the descending aorta, there being no valves in the uterine veins, and by the arteries being so weak, and their parietes so thin, that they cannot force the blood into the capillaries with the same rapidity as is effected in other parts, the consequence of which is that it accumulates in these vessels, and having acquired the character of venous blood, it exudes through their walls periodically. Although much which is here stated is merely the fruit of a lively imagination, and has no foundation in fact, and, at the same time, is at variance with opinions of Burdach in other parts of his work; it may not be unimportant to mention that the recent experiments of MM. Andral and Gavarret have been referred to as corroborating the notions of Burdach in regard to the small quantity of carbonic acid thrown off by the lungs in females. These authors assert, that they have ascertained, by experiments made on 26 females, that there is less carbonic acid exhaled by the lungs in females during the age at which the catamenia usually occur, than at an after period of life; and that even during pregnancy and amenorrhœa, there is always an increase of carbonic acid exhaled; thus showing that the catamenia assists the lungs in purifying the blood. These experiments, however, are far from being satisfactory, and they only tend to prove that young females exhale more carbonic acid from the lungs than those of middle age.*

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* "Apres la menstruation la femme jouit d'une pleine santé et du libre exercice de toutes ses forces, et son corps a le meme poids que quelque temps auparavant." Burdach, op. cit. p. 287.

† Op. cit., p. 287.

The notion entertained by Roussel, Nadow, and others, that the catamenia are the result of civilization, or that it is an hereditary disease promoted by the erect posture, is too absurd to require any particular notice.

A most material objection, and it applies equally to all the theories now enumerated, is, that they assume that the catamenial discharge is blood, and that it proceeds from a morbid action in the system, which is attended by a direct injury to the vessels of the uterus. Nothing, however, can possibly argue more thorough ignorance of the laws which govern all natural events, or is more at variance with all that we know of the provisions of Providence, than to ascribe to accident a function evidently so important in the female system. We find in other parts of the body every function admirably adapted for its purposes, and that there is none without an object, and it only requires the uterine system to be examined with sufficient care and minuteness to discover that it gives rise to no exception to the general law. Were the uterine vessels ruptured at every monthly period, profuse flooding would be of constant occurrence during menstruation. The very condition of the system which, by those authors, is considered favourable and necessary to produce the flow of the menses, would only add to the danger were a rupture of the uterine vessels to take place, and we should have the female exposed to the utmost danger at every menstrual period.

The theory which is now generally entertained with regard to the catamenial discharge is, that it is a regular and distinct secretion. The merit of having first suggested this explanation of the subject belongs to John Hunter; although claimed by Bordeu and Saunders. Mr Hunter had his mind drawn to this subject while investigating the principles of the blood, when he observed that the catamenial discharge did not coagulate. This circumstance particularly arrested his attention, and he alludes to it in the following words:—"In healthy menstruation the blood which is discharged does not coagulate; in irregular or unhealthy it does. The healthy menses, therefore, show a peculiar action of the constitution, and it is most probably in this action that their salubrious purposes consist; for if twice the usual quantity is evacuated with the power of coagulation even from the same vessels, the same benefit is not produced, much less when taken away from another part by art."[*] This important fact completely sets at rest the question of the plethoric theories, as well as the rupture of the uterine vessels. In another passage, in which he seems to have a doubt of his views being correct, he says, "The blood of the menses when it comes down to the mouth of the vagina is as dark as venal blood; and as it does not coagulate it has exactly the appearance of the blood in those where the blood continues fluid. Whether this arises from its being venal blood, or from its acquiring that colour after extravasation,—by its slow motion, it is not easy to determine; but upon being exposed it becomes florid; it is naturally of a dark colour, but rather muddy, not having the transparency which pure blood has. Whether this arises from its mixing with the mucus of the vagina, or from the cessation of life in it, I will not pretend to say. The red globules, however, are not dissolved, they retain their figure."[†] It was not until he had investigated the matter still farther that he declared his opinion that this discharge was a natural secretion, and he thus expresses his idea of the matter. "In a natural evacuation of blood, viz. menstruation, it is neither similar to blood taken from a vein of the same person, nor to that which is extravasated by accident in any other part of the body, but is a species of blood changed, separated, or thrown off from the common mass by an action of the vessels of the uterus similar to that of secretion, by which action the blood loses the principle of coagulation, and, I suppose, of life."[‡]

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* British and Foreign Review, January 1844, p. 257.

The immediate seat of the catamenial discharge has also been a fertile subject of controversy among authors from a very early period; and even yet the question is far from being completely settled. Some authors have asserted that it flows from the cervix;[§] others from the *fundus uteri* only;[‖] a third with equal confidence informs us that it is thrown off conjointly by the uterus and vagina,[¶] or entirely by the latter organ during pregnancy; whilst a fourth class affirm that its source is in the ovaries. Dr Raciborski imagines, along with Dr Power,[*] that there is a very intimate connection between the catamenia and Graafian vesicles, because the discharge begins when these have attained their developement and ceases when they become effete. He asserts that each menstrual period a vesicle swells and projects from the ovary, from which it escapes by rupture of its sac about the end of the menstrual period, without any influence of the male. That the cavity left by this vesicle has the same characters as the *corpus luteum* formed after conception. That disease arrests the developement of these vesicles, and that this is the true cause of amenorrhœa in many cases. That a similar developement and discharge of the Graafian vesicles take place in the lower animals during the rutting season without contact of the male, and hence that this period is the most favourable for impregnation in all animals, as near the time of menstruation is in women, especially the end of it. In proof of this he states that, "of fifteen women who specified accurately the period of their latest menstruation, as well as the dates of the connubial act, five conceived from coitus taking place from two days to four days previous to the period at which the catamenial discharge was due; in seven, conception was dated from coitus occurring two or three days after menstruation; in two, it took place at the actual period of the catamenia; and in only one so long as ten days after menstruation."—Lancet, 28th January 1843.

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[*] Palmer's edition of Hunter's Works, Vol. iii. p. 35.

[†] Op. cit., p. 85.

[‡] Ibid. p. 1 5.

[§] Columbus and Primrose.

[‖] Spigelius, Op. cit.; Mauriceau, p. 8; Litire Histoire de l'Academie Royal de Science, 1820, p. 20.

[¶] Burdach, p. 285; Desormeau, Dict. de Med. Tome xiv. p. 146; Velpeau, Tome i. p. 182; Forbes' British and Foreign Review, No. 7; Dewees's Midwifery, p. 118, eighth edition; Mayo, Physiol., third edition, p. 371; Astruc, Eng. Trans. Vol. i. p. 16.

Dr R. Lee,[†] whilst he denies that an ovum passes from the ovarium during menstruation, and that pregnancy takes place at that period, considers "that the phenomena of menstruation depend upon, or are connected with, some peculiar changes in the Graafian vesicles, in consequence of which an opening is formed in their peritoneal and proper coats." He quotes several cases in favour of this hypothesis; but although they give most satisfactory evidence in favour of the upper part of the uterus being the proper seat of the catamenia, and of the existence of a remarkable sympathy between this organ and the ovaries, they are quite inconclusive with regard to the opinion that a Graafian vesicle escapes from the ovarium at the menstrual period. These cases give no history of the women previous to their death; and they might be quoted with as much propriety in proof of pregnancy occurring during menstruation, which fact Dr Lee denies, as examples of a change taking place in the Graafian vesicles. Dr Lee further seems to think it "probable that menstruation does not take place in infancy because the ovaria are not then developed, and it is absent during pregnancy and lactation because at these periods they are in a quiescent state." But if the ovaries are not developed in infancy neither is the uterus, and he does not prove that these organs are in a quiescent state during pregnancy and lactation. Indeed the contrary is frequently proved with reference to the first condition by the existence of strong sexual desire during pregnancy, and the still more decided example of activity in the ovaries in the other, is shown by the occurrence of pregnancy in nursing women.

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[*] His Essay on the Periodical Discharge in the Human Female, 1832.

[†] Cyclopædia of Practical Medicine, Vol. iii. p. 227.

If we consider for a moment the structure of the organs thus assigned as the source of this discharge, we shall immediately perceive how inconsistent several of these views are, and how little they are supported by anatomy. That the catamenia have their origin in the cavity of the uterus is established beyond doubt by the discharge having been observed to flow from the *os uteri* when the organ was prolapsed.* This fact is farther corroborated by the greater tortuosity of the spermatic arteries, the swollen and softened state of the body of the uterus during the monthly period, whilst the cervix† remains in its usual pale and consistent state, at the same time we find the lining membrane much increased in redness and vascularity, and in addition to these circumstances, there are numerous well-authenticated cases in which the lining membrane is found bedewed with the menstrual discharge.‡ Of this the late Dr Hamilton, in his lectures, was in the habit of relating an interesting example, which occurred in a patient whose uterus was inverted, and during the monthly period the menses were observed to exude from the whole internal surface of the cavity of the uterus. If any doubt should still remain on this subject it must be entirely removed by the following cases, the first related by Cruveilhier,§ of a woman in whom the body of the uterus was found awanting on dissection, but the cervix was entire: this woman never menstruated. Another case is related by Seguin of a healthy woman, 27 years of age, who had the mammæ and external organs of generation well developed, but the uterus was absent, the vagina was a mere sac about an inch and a half in depth. She was married, and manifested sexual appetite, thus indicating the presence and integrity of the ovaries. There was a slight increase of moisture in the genital organs every month, but there was no menstrual discharge, nor any secretion answering that end.‖

That a coloured discharge flows from the vagina occasionally during the early months of pregnancy, and sometimes throughout the whole period of gestation, is a well-known fact, and must have been observed by all practitioners of midwifery; but that this discharge is the regular catamenia I am far from admitting. It is true that in some instances such a discharge returns periodically, and in this respect it resembles the natural discharge; but here, so far as my experience goes, the analogy ends; as in all cases I have found that the fluid thrown off differs in many particulars from the regular catamenia at other periods. It is almost invariably accompanied by clots, and is either more or less in quantity than the menses usually are in the same individual. We are therefore justified in supposing that it is merely a discharge of blood, arising most probably from some change in the mucous membrane of the vagina, produced by increased vascularity which pregnancy occasions in the uterus and the neighbouring parts.

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* Russel gives an excellent illustration of this in his works.

† Cruveilhier's Anat., Lib. of Med., Vol. viii. p. 120.

‡ Cyclopædia of Practical Medicine, Vol. iii. p. 227.

§ Cruveilhier's Anat., Lib. of Med., Vol. vii. p. 120.

‖ Lancet, 26th November 1842, p. 316.

That the catamenial discharge is a natural secretion having its source in the glands of the lining membrane of the uterus, and that it proceeds from a healthy action, and is not the result of disease or injury in the vessels of the organ, can no longer admit of a rational doubt. But whilst this is an obvious fact, it must also be remembered that there is a peculiar state of the general constitution which attends the flow of the secretion, the precise nature of which is little understood, and cannot be explained upon the principles of mere plethora. This peculiarity is most conspicuous in those women who menstruate during lactation, and is observed only by its effects on the child. We generally find the child suckled by such women remarkably pale, and it sometimes has a yellow-waxy appearance. It is feeble, flabby and ill nourished; and at every monthly period it is attacked with colic pains, and sleeps ill, the alvine dejections are more frequent than usual, and are of green colour. These symptoms are sudden in their attack, and there is no apparent cause for them. Their true nature is only discovered by a strict investigation into the condition of the nurse, who too frequently takes every means to conceal that her catamenia have returned. In such circumstances I have found much difficulty in persuading mothers what was the cause of their child's illness, and the consequence has been that an infant has been exposed for months to a succession of such attacks as I have described, until by some accident the actual state of the matter has been ascertained. M. Brierre de Boismont mentions several cases which illustrate the bad effects of menstruation during lactation. One case, in which the woman was regular every three weeks, whose child died. Another, where the woman menstruated during eight pregnancies as well as while suckling. The whole of her children died before their fifth year.

In the healthy female the catamenial discharge is thrown off every four weeks, and, with such regularity in some women, that they can reckon to within an hour of its return. This is a law which once established, seems to regulate all the operations of the uterus; hence we find that parturition usually is considered to come on at a menstrual period. But why this law should limit the return, in general, of the menses to every twenty-eight days has puzzled all who have written on the subject, and it is likely to continue to do so unless it may be explained on the principles so clearly laid down by Mr Goodsir,* as regulating other secretions. Mr Goodsir proves in the most satisfactory manner that secretion takes place on free surfaces, from nucleated cells, which burst when they attain a certain state of developement. Are we not justified, therefore, in thinking that the interval between the return of the catamenia is merely the period required for the ripening of these secreting cells in the lining membrane of the uterus?

On its first appearance the secretion is small in quantity, and in some females it is colourless; but in the course of a short time, when it becomes fairly established, it attains to its natural quantity and colour. Its quantity varies in different climates, and, sometimes, even in the same woman at different periods. In the temperate climates of Europe its quantity is from six to eight ounces; but in some parts of Greece, it is stated, on the authority of Hippocrates, to amount to eighteen ounces; whilst in the cold northern regions it does not exceed two ounces; and according to Linnæus, in those regions it flows only in the summer months. In both of these statements there is probably some inaccuracy, and I am disposed to think there is no better foundation for the latter statement than for that of Boerhaave in his Lectures, p. 117, in which he asserts that the women of Scythia, the Brazil, and Greenland are totally without this discharge—or of that of Emmet, Roussel, Aubert, who consider that the catamenia are the result of civilization.

The regularity in its return, however, is far from universal, and some authors state that it sometimes returns every fortnight, and that this is frequently hereditary,† and in others it returns only once in six weeks, or every two or three months‡ without giving rise to any inconvenience to the system. Aristotle even asserts that it occurs only every three months. But at whatever period it returns it never, in its healthy state, flows in a continued stream nor ends suddenly. It is discharged drop by drop, gradually increasing in rapidity from the first to the second or third day, according to the period it usually continues, and diminishes in the same gradual manner until it entirely ceases for the time. Its usual duration is five or six days; but sometimes it does not continue above three days; on other occasions it exceeds seven or eight days[*] without producing any inconvenience to the health. In some women it is both preceded and followed by a pale mucous discharge which lasts for a day or two, but in general requiring no particular attention.

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* See his paper in the Transactions of the Royal Society of Edinburgh, 1842.

† Roberton on Menstruation, Edin. Med. and Surg. Journal, Oct. 1832.

‡ Velpeau, op. cit. p. 125–6.

In the temperate climates of Europe the catamenia make their first appearance most usually between the thirteenth and fifteenth year; but in Africa, in many parts of Asia, and in other warm countries, it is generally believed they commence as early as the eighth year, and we are informed that in China it is no unusual circumstance to see mothers not more than twelve years of age.[†] This is an event, however, not entirely confined to China, as Haller states, "In puella annorum novem exierunt menses, et utero concepit;" and Mr Roberton[‡] relates the case of a girl near Manchester, who, while only eleven years of age, was confined of a still-born child at the full period. She menstruated before she became pregnant. In Lapland and other cold countries, on the other hand, it is supposed that the catamenia do not appear before the eighteenth or twentieth year.

Much difference is occasionally observed with regard to the first appearance of the catamenia, not only among women in the same climate, but even in the same locality. This arises very probably from temperament, education, mode of life, and conversation. Repeated instances have come under my own observation of women in whom the catamenia had appeared before their eleventh year, and continued to return regularly afterwards. In a case which came under my care lately, the catamenia commenced, the woman informed me, in her tenth year, at which period she said she was large of her age, and they entirely ceased when she attained her thirty-seventh year. This individual had only one child and one miscarriage. During her monthly periods she was subject to a stinging sensation in her breasts, which had continued to return regularly, although she had had no catamenial discharge for three years when I last saw her.

In Dr Brierre de Boismont's interesting treatise there is given a statistical account, drawn up from the examination of 1200 women, which illustrates in a striking manner the effects of temperament, locality, and habits on the first appearance of the catamenia. In 477 females of the lower classes, he found that the catamenia appeared earlier in those of a sanguine temperament, and latest in those of a lymphatic temperament. In the former, the age at which the discharge appeared was 14.578, while in the latter it did not appear until 15.381. In women having fair or red hair the appearance was more tardy than in those of a dark complexion, and later in tall women than in those of small stature. He likewise found a slight difference between those who lived in towns and those who lived in the country. In towns, the mean age for the appearance of the catamenia was fourteen years and six months, whilst in the country it was fourteen years and ten months. He found a remarkable distinction, too, in the different ranks of society. In the poor classes, the mean age was fourteen years and ten months; in the middle classes, which he calls the *metis* " *mulatto* or half-cast," fourteen years and five months; and in the highest classes, thirteen years and eight months. In robust and healthy women, who take much exercise, the catamenia usually appear during the day; amongst the delicate, the discharge appears more generally during the night.

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[*] Brierre Boismont op. cit.

[†] Moreau, Hist. Nat. de Femme, Tom. i. p. 143.

[‡] Ed. Med. and Surg. Journal, Oct. 1832.

Mr Roberton, in an admirably drawn up paper in the Edinburgh Medical and Surgical Journal,* denies the correctness of the latitude usually assigned, to which we have referred above, for the appearance of the catamenia in women in different climates, and from the following table, drawn up from the women who applied to the Manchester Lying-in Hospital, he was disposed to think that the natural age of puberty of the women in this country, instead of being the " fourteenth or fifteenth year, occurs in a much more extended range of ages, and is much more equally distributed throughout that range than authors have alleged."

Of 450 women	10	began to menstruate in their	11th year.
	19	- -	12th
	53	- -	13th
	85	- -	14th
	97	- -	15th
	76	- -	16th
	57	- -	17th
	26	- -	18th
	23	- -	19th
	4	- -	20th

Mr Roberton is inclined to believe that puberty appears pretty much at the same age in all climates, and that the apparent difference arises from the early marriages which are common in many warm climates, and especially in Asia, where, among the high-cast Hindoos, women are not considered marriageable after puberty appears; and from the universal licentiousness in those climates—leading to very early sexual intercourse—a circumstance which the higher notions of morality do not admit of in civilized countries, otherwise mothers at the age of twelve would be more common in our own country than they are. Another cause of error in this investigation, he considers, is to be found in the difficulty of ascertaining the age of women among barbarous nations, arising partly from their becoming bewildered after a few harvests have passed, and unable to recollect the year in which they were born. In other instances, from superstitious notions preventing the people from noting their ages at all.

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* Oct. 1832.

A remarkable precocity in the appearance of the catamenia, and of some of the other signs of puberty, is mentioned by some authors. Dr Martin Wall* relates the case of a child nine years of age, who had menstruated regularly from the age of nine months, and possessed many of the outward signs of puberty. Haller mentions the case of a child of three years of age, in whom the menses flowed very profusely, "menstrua magnâ copiâ in trienni puella fluxerunt."† Cases not less remarkable are mentioned, in which there is an unusual tardiness in the appearance of the catamenia. Haller refers to several instances of this kind, in some of which the discharge did not appear till very advanced life. "Sic mulier quædam anno ætatis centesimo primo menstruum fluxum excepit, eundemque cum emolumento valetudinis tribus annis servavit." Again, "Nobilissima mulier anno septuagesimo recuperavit menses."‡ But it is unnecessary to quote more cases in example of this precocity and tardiness in the appearance of the catamenia, as they are all to be considered as the result of a morbid action of the system. With regard to the early appearance of the catamenia, Sylvius§ and many of the older writers imagined that those in whom they appeared soon were short-lived, and those in whom they were late of appearing attained a greater age.

Thirty years seem to be the time generally allotted in all countries for the continuance of the catamenial secretion. Hence in this country it usually ceases to return after the forty-fifth year; although cases are not rare in which it continues beyond the fiftieth year; and instances of its continuing till the sixtieth year are related by some authors. "Menstrua ad sexagesimum servantur quo tempore etiam nonnullæ pepererunt; sed ultra hoc tempus nullæ potest menstruam purgationem protrahere." I am disposed to think, however, that when a coloured discharge occurs from the private parts after the fiftieth year, it is not to be considered the catamenia, but simply a discharge of blood, many instances of which have come under my observation in women who have had large families. It is not regular in its return, and is always attended with a greater or less degree of weakness and loss of appetite; occasionally I have observed it accompanied by pain in the right hypochondrium, and with purple spots in different parts of the body. On examination *per vaginam*, I have found the womb enlarged, but not painful, and the blood coming from it of the bright colour of arterial blood. In such cases I have observed advantage to be derived from nourishing diet and tonics.

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* Trans. of Med.-Chir. So. of London. † Disput. Anatom., Vol. v. p. 203.
‡ Op. cit. p. 204. § Oper. Omnia, p. 169. Also Haller, op. cit., p. 203.

Notwithstanding what has been said of the virulence of the catamenial discharge, it seems in its natural state to be a bland and harmless fluid, neither irritating the passages it flows through, nor the other parts it comes in contact with. It is dark in colour, resembling the venous blood ; but is thinner, and the stain which it leaves on linen more easily removed. Velpeau* asserts that its colour is always pale in the commencement, and towards the termination of the monthly period ; that it is darkest at the middle of the monthly period, at which time it is like the blood which flows from the nose in epistaxis. I am inclined to believe, however, that this is not universally the case, and that although it becomes generally pale towards the termination, the first drop is frequently as dark as any that flows throughout the whole period, after puberty has been fairly established. Hence the first indication which the woman has of becoming "unwell," in many instances, is observing the dark stain on her linen. But there are exceptions to this, and women are occasionally annoyed by a leucorrhœal discharge for a day or two both before and after the regular secretion. In its healthy state it does not coagulate under any circumstances. This property has been ascribed to its having a small proportion of fibrin, which, at the same time, enables it to resist putrefaction longer than pure blood,—a peculiarity which has been ascertained by its remaining fluid, and undecomposed for years within the uterus, in imperforate hymen, and in occlusion of the *os uteri*. Dr Retzius denies that these properties depend on a want of fibrin, and consequently a deficiency of azote ; because casein, which, of all substances, with the exception of urea, contains the largest proportion of azote, and resists putrefaction when even in the most favourable circumstances for producing it. He asserts that the menstrual discharge contains the same colour, number, and form of globules as the blood, but that it differs from common blood in possessing stronger acid properties. That this acidity depends upon the presence of free phosphoric and lactic acids ; and that hence arises the non-coagulation of the catamenia.† The description here given of the appearance of the catamenia is supposed, in some measure, by Mr Brande,‡ who, although he was unable, in his experiments on this discharge, to discover any globules ; he nevertheless considers that the secretion possesses "the properties of a very concentrated solution of the colouring matter of the blood in a diluted serum." The acidity mentioned by Retzius as existing in the catamenia is supposed by some authors to proceed from the mixture of mucus from the vagina and neck of the uterus, which M. Nauche has proved to be acid in a healthy woman after delivery. But when it is glairy, or the product of inflammation, it is alkaline[*]

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* Op. cit., p. 124.

† Brit. For. Med. Rev. July 1836.

‡ Phil. Tr. 1812, p. 113.

The following analysis of the catamenia is given by M. Bouchardat.[†]

Water,	90·03
Fixed matters,	6·92

The fixed matters were thus composed.

Fibrin, albumen, and colouring matter,	75·27	Salts,	5·31
Extractive matter,	0·42	Mucous,	16·79
Fatty matter,	2·21		100·0

Another specimen of the same menstrual fluid was examined by M. Donné,[‡] who observed the following microscopic characters:—1. Abundance of the ordinary globules of the blood; 2. Vaginal mucous with epidermic squamæ, from the mucous membrane of the vagina; 3. Mucous globules furnished by the neck of the uterus.

These investigations, although they may be correct, as to the chemical characters and microscopic appearances of the fluid submitted to examination, they leave the question with regard to the component parts of the healthy catamenia undecided; because the fluid subjected to analysis and to microscopic observation, we are informed, was taken from a woman of delicate frame, who had been kept chiefly upon vegetable diet. To this circumstance M. Bouchardat ascribes the great proportion of water found in the fluid. We cannot, therefore, consider this to be a satisfactory analysis of the menstrual fluid, as there can be no doubt that the state of the general health will affect in a greater or less degree the composition of this secretion.[§] Besides these circumstances, this analysis is defective in having left undefined the proportions of fibrin, albumen, and colouring matter, found in the fluid, and in having merely stated, that there were certain salts, without specifying what those salts were.

With regard to the microscopic investigation of M. Donné, it is not more satisfactory, as it is very questionable if the fluid examined was the menstrual secretion at all. At all events, if it were the catamenia, it must have been so carelessly collected and seems to have contained so many extraneous matters, that we cannot look upon it as giving a correct example of the secretion. Had due attention been paid to obtain the fluid from the uterus, we should have had no "vaginal mucus formed of the epidermic squamæ from the mucous membrane of the vagina."

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[*] Med. Chir. Rev. April 1842.

[†] De la Menstruation, par A. Brierre de Boismont, p. 273.

[‡] Brierre de Boismont, op. cit. p. 174.

[§] This opinion is farther supported by Andral in the following words: "As the blood nourishes the solids, and as without its presence they cannot support life, the state of the solids cannot but be influenced by the state of the blood. Treatise on Pathological Anatomy, translated by Drs Townsend and West, Vol. i. p. 642.

Müller believes that the catamenial discharge differs from ordinary blood only in possessing less fibrin, or none at all, and therefore that the blood corpuscles exist in a natural state in this fluid.*

We thus see that our information with regard to the chemical composition of this secretion is vague and undecided, and that before correct conclusions can be come to on the subject new experiments must be performed, and that the catamenia for such a purpose must be obtained either from the prolapsed or inverted uterus.

The catamenia have had various uses assigned to them by different authors. The ancients, forgetting the virulence which they had ascribed to this secretion, believed that its object was to nourish the *fœtus in utero.* Modern authors oppose this theory principally on the grounds that there is not sufficient fluid thrown off in the form of menses to nourish the fœtus during the nine months' gestation. Many authors agree with Denman and Ramsbotham† in imagining that the principal use of this secretion is to preserve the uterus in a fit state for conception.

Neither of these theories is satisfactory. That of the ancients is inconsistent and absurd, and is opposed even by their own arguments on the nature of the discharge, as well as by the fact that the organization of the fœtus, even in the latter months of gestation, does not admit of its receiving and assimilating the menses in the manner they supposed. The notion of the modern authors is not more tenable. It takes the effect for the cause. As well might it be said that the secretion of the gastric juice is the cause of the stomach being in a healthy state, as that the flowing of the menses is the cause of the uterus being healthy and fit for conception. The regular secretion of the menses is a strong indication of the uterus being in a state of health; but it cannot be considered the cause of this state; neither is the regular return of the catamenia always a sure sign that the uterus is in a fit state for conception, as is proved by the fact that many women who are quite regular in this respect are sterile. Again, there are many cases on record of women having their uterine system apparently in the highest state of health, yet not have their menses for years, in consequence of their either being pregnant or suckling. An example of this kind came under my care lately. This woman had been married six years, and was then pregnant of her fourth child. She had suckled all her children fourteen months, and had not menstruated since her marriage, yet she had always enjoyed good health.

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* Muller's Physiology, Vol. ii. p. 41.

† Denman's Midwifery, Vol. i. p. 171, and Ramsbotham's Lectures, Med. Gazette, Nov. 23, 1833.

The catamenial discharge has an obvious reference to the important functions of gestation and lactation in women; and the most rational theory with regard to its use is, that it establishes a principle in the female system, which enables her to bear the drain of supporting the *fœtus in utero*, as well as suckling the child, not only without injury to her constitution; but in numerous instances with advantage. It may be objected to this hypothesis that no such principle exists in the lower animals; but their constitution and mode of life render such a provision totally unnecessary.

The age at which the catamenial secretion first appears, and that at which it finally ceases are two of the most important epochs in the female existence, and they have in consequence been called the critical periods; because at these periods diseases seem to take their origin which often embitter the existence for the remainder of life—if they do not prematurely cut it short. We do not, however, discover that the final termination of this secretion is a period more fatal to women than the same age usually is in man. The age of puberty, as it is called, at which this discharge first appears, is also accompanied by many important mental and bodily changes. Its approach is indicated by increasing reserve in the young female, which induces her gradually to withdraw from the companions and amusements of her childhood, and to attach herself to the society and occupations of older people. Her mind now becomes stronger; her ideas more womanly, and her whole manner assumes a more graceful and dignified demeanour. Her figure is now more fully developed, and all the individual organs of her system acquire a more perfect form. Her face is marked by higher intelligence, and her eyes are illuminated with a peculiar brilliancy. The mammæ are enlarged and prominent; the *mons veneris* and the pudendum become covered with hair. Hence the term puberty which has been assigned to this period. Her voice loses its girlish shrillness, and acquires a soft and melodious tone. Whilst these outward and obvious operations are going on, others not less important, although less apparent, are accomplished internally. Her whole system seems now to be perfected, and she is prepared for the important functions of conception and affording nourishment to the child.

All these changes may be effected, and the first appearance of the catamenia may take place without any morbid symptom; but very generally the first secretion is immediately preceded and accompanied by listlessness and headach, loss of appetite and fœtid breath, dulness of the eyes, which at the same time becomes surrounded by a bluish circle, fulness and pain in the breasts, pain in the back and loins, irritation of the bladder and vagina. These symptoms are usually more severe at this than at the succeeding periods, but in some women they are equally severe at every menstruation. As, however, they are in general only temporary, women seldom apply for medical aid on their account. It is only, therefore, by their continuance, or their becoming more violent that the physician is made aware of their existence.

When we consider all the important attendants on this most interesting event, it is not surprising that parents and guardians should watch with great solicitude the approach of this period, and it is therefore necessary that the young female should be prepared for the great revolution which is about to take place in her system, and guarded against every thing which is likely to interrupt the accomplishment of these changes. At this period the utmost attention should be paid to the diet, the clothing, and the exercise of the young female, and the slightest tendency to constipation should be corrected. At the same time, however, hash and severe medicines are to be avoided.

Even after puberty is fairly established and the catamenial discharge has commenced, young females shonld be warned against every thing which is likely to excite or to depress them, such as dancing, long walks, exposure to cold or damp, at the approach or during their monthly periods, because there is reason to believe that the female constitution is more susceptible of disease at the commencement of, and during the periodical discharge, than at any other time, and in some instances the system becomes so irritable as to be affected even by the slightest impressions. These directions are also to be carefully observed near the final cessation of the discharge, as at that period the diseases peculiar to the mammæ and uterus are particularly to be dreaded.

(To be continued.)

Art. IV.—*Medical Notes on Syria; or Practical Observations on the Diseases there treated during a residence of thirteen months, including Remarks on the Country, its Climate, People, Resources, and an Account of the Plague as it occurred in Bairout in* 1841. By Ebenezer Robertson, Lic. R. C. S. E., Surgeon Royal Navy, and Medical Officer attached to the British Staff and Ordnance Detachments serving in the expedition to Syria in 1840 and 1841.—In a Letter to Sir William Burnett, Kt. M.D. (Continued from Vol. lx. p. 73.)

The Plague. Introductory Remarks, &c.—This disease may be viewed as an endemic confined to Turkey and Lower Egypt. Although it seems to be peculiar to those countries and provinces in sporadic cases in all seasons and under all circumstances, yet there are only certain seasons during which it can exist as an epidemic, and a certain period of those during which it can present itself in severity; its appearance, virulence, mitigation, and final disappearance for the time being evidently regulated by temperature. The disease can only exist in a temperature between 60° and 80° Fahrenheit,—a lower or higher degree either modifying or utterly destroying that atmospherical constitution, or those other occult causes giving rise to its origin and propagation.

In the lower countries and plains we find its power either mitigated or entirely abolished by the 24th of June; which day is generally ushered in by the roar of cannon and by the rejoicings of the people. In the more northern parts and in the mountain ranges, cold is the only power on which the inhabitants place reliance for being able to combat this scourge in its progress, or by which to effect its expulsion. There is one other circumstance which we would mention, a fact which we find committed to record by Celsus, and one as true in our day as it was in his, that this disease when it comes from the north is much less dreaded and feared than when it has its origin from the south. The latter, however, is the more frequent source of its appearance, it having been found in a very few instances only to have travelled from the north to the south. So perfect a conviction of this circumstance have the natives, that "when the plague is at Smyrna, the inhabitants of Aleppo handle goods without precaution, ;" whereas, "when the disease is at Damascus, great precautions are observed, and all the Frank families hold themselves in readiness to shut up or to leave the town." Our own experience of the feelings of the people perfectly coincides with the opinion of Russell here quoted.

Before proceeding to describe the plague, as it occurred in Baïrout in 1841, to which I shall entirely confine myself, I shall offer a few remarks on the origins assigned to it, its gradual progress in the country, and, to render my description more clear, premise them by a few observations on the town of Baïrout itself.

In the latter part of December 1840, or early in January 1841, the plague "demonstrated itself at Tyre, amongst the Turkish soldiery in garrison. Two very probable origins were given; the one,—that it had been imported from Egypt, where the disease was raging, by a merchant vessel, which, without undergoing quarantine, had communicated with the town; the other,—that it took its origin from some villages in the district of Belid-El-Scherif, in the neighbourhood of Tyre, amongst the inhabitants of which district the disease had committed extensive ravages the preceding spring,* 1840, and no care had then been taken to disinfect either the houses or the wearing apparel of those who had fallen victims to it. Of the two, the former, the more probable, was most generally credited by those who were living in the town and neighbourhood. Military movements being at that season† absolutely necessary, and a concentration of all the Turkish army being called for at Jaffa, small garrisons were left in those towns, to the northward, which were already in possession of the Turkish power, and the rest were marched to the southward;—these were permitted *en route* not only to communicate with, but even to enter the already infected town of Tyre. To such did the fatalism of the Turkish leaders urge them, that they were not contented with permitting the admixture already taken notice of, but even ordered the very regiment in which the disease had first demonstrated itself to march, but shortly thereafter to join, and to become incorporated with the whole Turkish army encamped around and billeted in Jaffa.

As was to be anticipated, the troops were not long assembled before the disease made its appearance amongst them. For some time it confined itself entirely and solely to the military and those civilians attacked, but it soon extended itself to the inhabitants of the town and neighbourhood. At this critical moment, when we were watching the retreat of Ibrahim Pasha, the English staff-troops were in Jaffa, the British commanding officer, the late Colonel E. Bridgeman, ordered the reported cases to be examined by Deputy-Inspector-General Robertson, who had arrived a few days previously from St Jean d'Acre, where, at the risk of his own life, he had for weeks taken charge of the many sick there, and at the convalescent hospital at some distance, the two medical officers‡ thereto belonging, having been obliged to be removed, having contracted the then prevailing fever. By reference to Vol. ii. Levant correspondence, we find there inserted Dr Robertson's letter of date February 15, 1841—a copy we subjoin. "I found three men labouring under disease, which I am decidedly of opinion is plague, and there are several others with suspicious symptoms. These men were admitted into hospital from this garrison, and there is therefore every reason to suppose that the disease may further develop itself among the troops in Jaffa." In consequence of the letter just quoted, the presence of the British detachments being no longer necessary, Ibrahim being on the eve of embarking, they were ordered to Baïrout; which town had not yet suffered. The Turkish troops were ordered, at the same time to march; their object, however, was the garrisoning of every town to the northward. No care was taken prior to, or during their march, to separate the healthy from the diseased; and they carried the disease every where; it followed their march, in which many were attacked, and struggling, died at the roadside, uncared for and unmissed. Shortly after the arrival of the separate companies at their individual posts, the disease broke out; it confined itself in all cases, for some time, to the persons of the soldiers or of the attendants upon the officers. The following is an outline of its course.

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* Vide Appendix, 7.

† December 1840 and January 1841.

‡ They both ultimately recovered.

First, At St Jean d'Acre, where so many of our gallant marines fell victims to fever, late in January and very early in February.

Secondly, At Sidon, in the persons of two secretaries attached to the commanding officer there, towards the middle of February.

Thirdly, At Baïrout. As before noticed, immediately after the letter of Dr Robertson already quoted, the British detachments were moved back to Baïrout. On our arrival there fears were found to exist, partly from the season in which it pays its visit fast approaching, and partly from the daily received exaggerated reports of the progress and fatality of a scourge which the natives hold in great dread and terror.* Every illness was eagerly and inquisitively looked upon, with a view of discovering some of the unequivocal marks of the disorder. They were not long kept in doubt. On the 27th of February it declared itself in the person of one of the attendants of his Excellency, Omar Pasha—which personage, with his suite, &c. had, only a few days previously, arrived from the south, having communicated, *en route*, with the already-mentioned intervening affected places.

Finally, It broke out at Caiffa, various villages along the seacoast, and the lower ranges of the mountains, extending gradually to the north; in 1841 it did not proceed beyond Latakia, and for several previous years it has not been known in either Aleppo or any of the other towns or villages to the northward, or in their vicinity. It continued its ravages with greater or less violence in the various parts already mentioned, besides numerous others requiring no notice, till about the middle of May, when, with the increasing temperature, it became much modified, was attended with much less mortality,† and by the 14th of June had disappeared in Baïrout, &c. It still existed, however, in the lower ranges of the hills, where as yet the heat had not become too intense for it. In the higher parts of Lebanon it exists, as already hinted at, to a greater or less extent throughout the whole summer. At visits I paid to the district of Mazzra, (about 3000 feet above the level of the sea), in the months of September and October, I found it then and there prevailing. The inhabitants of that district, on being interrogated as to the probable time of its subsidence, answered, "when the water was frozen," thereby signifying when the cold had set in.

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* In speaking of natives, in reference to the plague, we except the Moslemin, who, from their religious belief, have no fear or dread. The better educated are not so blindly attached. See subsequent remarks.

† See Table, App. 4.

Baïrout, a seaport town, in latitude 35° 50′, longitude, 35° 28′, is situated upon the northern side of a tongue of land which stretches out in a westerly direction from the base of Anti-Lebanon, from which and the remainder of the mainland, it is separated by Baïrout River, which forms its eastern boundary; to the northward and westward its shores are washed by the sea; the latter presenting a large mass, or rather plain, of moving sand, has gradually encroached with each succeeding westward blast upon the houses and gardens in the suburbs of the town, and involved all in one common ruin. It has already converted what was formerly an extensive garden, studded with neat eastern cottages, into a barren sandy waste, which in its turn threatens to sweep by its onward course all signs of vegetation or animation from the adjoining surface. To the southward it is continuous with the plain which stretches towards Sidon, intercepted, however, by a small rising hill, the most prominent part of which is termed St Demetrius. The town itself, inclosed by a fortified wall, is situated about one-third distant from the extreme point of the tongue, "Ras Baïrout," occupying a very small portion of the coast, but extending some distance inland. The houses, which are numerous, are thickly built and densely inhabited; the streets are narrow, and in the generality of places, like other Eastern towns, in a state of disgusting filth, and the open common sewers, running through the very centre of their bazaars, reek with decomposing masses of animal and vegetable matter, tainting the air to a great extent. The population, composed of Turks, Syrian Christians, and a few Europeans, is estimated at about 10,000 souls. It is thought by the native inhabitants to be one of the healthiest low towns in the country. The mortality we would state to be about three per diem, or 10 per cent., but this includes all who die of epidemics, &c.

Considerable mercantile transactions are carried on here; it may be considered to be the first seaport trading town in Syria, and in all probability will continue to increase in importance were tranquillity restored to the country.

The suburbs, which are extensive, in summer present to the eye one continued range and expanse of mulberry gardens; they are reckoned to contain about 8000 souls, thereby making the total of our included district 18,000 souls. Their principal occupation consists in rearing the silk-worm, and preparing the raw material for either home use or for exportation. Of late several very extensive establishments have been erected in the neighbourhood for the purpose of winding the silk for the European markets. What we have already said in relation to the seasons, temperature, habits of the people, &c., applies equally to this town. The water in the town is exceedingly brackish, and during the latter part of summer and autumn is incapable of being drank; but there are in the neighbourhood large tanks and numerous springs, from which supplies are continually brought in skins or in jars on donkies. The water of the Dog River, Nahr el Kelb, and of a small stream between it and Bairout River is wholesome; the water of both is used by the shipping; George's Bay, as already stated, forms the only safe anchorage for vessels during the winter season.

The Plague (Humor Jack, Turk. Tãoon, Arab.), as before stated, made its appearance in Bairout, on the 27th of February 1841, in the person of one of the attendants of his Excellency, Omar Pasha. Shortly after the Pasha's arrival, this individual was suddenly seized with vomiting and the other usual premonitory symptoms of fever. The case, from the various circumstances we are about to mention, was looked upon with a very suspicious eye. His arrival from the southward, his being seized with vomiting, and it being the season in which this disease appears,—all excited the fears of the sanatory establishment, and he was immediately inspected by the medical officers; doubt took possession of their minds, and in their uncertainty they recommended that surveillance should be observed; in consequence of which decision, the sufferer, with the other attendants in company with him, was submitted to observation, guardians being placed over them and their necessaries, so as to obviate any intercourse till such time as the disease should develope itself. The second day removed all doubts, and the disease was declared to be the plague. Immediately after the sanatory inspection, the individual in question, with those already mentioned, and all their baggage, &c. was directly conveyed to the Lazaretto,* a building about two miles from the town. The next cases were individuals landed at the Lazaretto, to undergo quarantine, having arrived in a bombard from St Jean d'Acre; thereafter in the persons of passengers from Alexandria; the Turkish troops presented the next subjects; it attacked them at their posts; the sick in their hospitals, and by the first week in April it had extended itself to the inhabitants in the town and the suburbs; by the 13th day of June it had ceased, there having been in all 121 cases, of which 47 were fatal.

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* See description of the Lazaretto subsequently.

Though the disease varies considerably in its symptoms, I would define it to be "An exanthematous disease, the eruptions consisting of buboes, carbuncles, gangrenous pustules, and petechiæ, occurring singly or variously combined, attended generally, but not invariably, by fever, which varies in intensity and degree; but whether mild or severe, always of an adynamic type.

General Description.—All inquiries failed to elicit from individuals attacked by the severe form of the disease any of those premonitory symptoms which characterize the onset of any of the severe diseases of our northern latitudes; the person being suddenly seized, without any warning, with nausea, general uneasiness, debility, lassitude, inclination to drowsiness, dull pain and weight in the head, a feeling of stupor and inability to fix the attention. In a few instances there are marked or ineffectual attempts at rigors, uneasiness of præcordia, often attended by vomiting of bilious or very offensive fluid; the surface of the body looks bloodless, and to the patient's sensation and to that of his attendants, feels cold and moist. There is great desire for fluids and to urinate. This state is shortly succeeded by an increase of all the complaints and sufferings; the face assumes the appearance of one intoxicated, but is pale; the eyelids are partially open; the eye dull and devoid of intelligence; the breathing oppressive; the mouth clammy; the odour of the breath heavy and offensive; the præcordial sensations now amount to pain, giving rise to frequent ineffectual attempts to vomit, which both harass and exhaust the patient; the depression becomes extreme; the head falls back; the tongue can with difficulty be protruded; when effected, it is done with a tremulous motion; the limbs no longer obey the will and refuse to support the body; if he be supported, and induced to endeavour to walk, the head drops on the chest, he shuffles with an instable gait, his whole body inclines forward, and when laid down he lays as he falls, appearing quite unconscious; the eye looks dead and dull; the pupil widely dilated; the urine passes in large quantities, and is highly offensive; the bowels generally confined; they will be found not only to have been so for some time previously, but from the impaired sensibility of the whole system, can with difficulty be persuaded to render their contents. Diarrhœa sometimes exists; when it does, it may be looked upon as a very unfavourable symptom.

This stage, which may be called the first, or stage of collapse, lasts in all its varying forms and degrees for a period varying from twelve to eighteen hours; and in the event of death not taking place in it, a circumstance which frequently happens, it is succeeded by the second, or that of reaction. The commencement of this stage is indicated by the individual evincing symptoms of uneasiness; there is a "moving about," conveying the impression to the beholder that the individual is endeavouring to obtain, by continual change of position, relief from some internal disagreeable or annoying sensations, which he cannot explain. A slight blush gradually suffuses the cheek; the eye, of which the pupil is widely dilated, becomes vivid and lively, but perfectly void of intelligence, giving, when contrasted with the countenance, a most peculiar and indescribable appearance, requiring only once to be seen never to be mistaken or forgotten; the lips become parched, the tongue begins to swell and to become dry, and, whilst furred or thickly coated in the centre, has its edges red and thickly studded with elevated papillæ, which, mingling with the margin of the coating, gives the same peculiar appearance of the tongue which is observed in other exanthemata; the breathing is short and anxious; the præcordia more and more painful, and the whole is described as 'twere in fire, producing great desire for cold fluids, which are greedily swallowed, but afford no relief; the headach changes its character; it now is no longer dull and confused, but acute, giving rise frequently to typhomania; there is more or less low muttering, with convulsive muscular action, more especially of the facial muscles; sleep is rarely obtained; if quietness, simulating sleep, should for a time exist, he suddenly awakes, starts up, looks wildly around him, and feels not at all refreshed; the skin is described as burning; the urine, small in quantity, is high-coloured, offensive, and excites pain on being passed—it is sometimes suppressed. On being interrogated, he answers by a vacant unintelligible stare, or if capable of answering, it is effected by great exertion, in a very low and feeble voice; he points to his head and to his stomach; the latter he presses firmly with both hands, and drags out, "nar honīki," "fire here," and seems quite overcome by the exertion. Spasmodic twitchings now take place in the arms and groins, and pains and itching are complained of in the other parts, where the eruptions are about to present themselves. If vomiting still continue in this stage, the ejections will be found to be highly bilious and offensive; so also the dejections, if the bowels have been relieved; the tongue becomes, toward the end of this stage, very dry, and not unfrequently shrivelled up, chapped, or cleft; the teeth and lips become covered with sordes, or a matter of a dark glutinous character; the inflammatory action can now be observed in the situations of the glands or other parts, where either bubo, carbuncle, or pustules are to appear; epistaxis may take place, in some few cases in which it did occur, it afforded relief. This stage, about the fortieth or fiftieth hour, yields to a perspiration, which was described by the attendants to be very heavy and offensive;

the febrile action gradually subsiding. The individual becomes less and less restless and uneasy; the countenance gradually acquires a more natural expression; the eye becomes less vivid and void of suffusion, if the previous re-action have been sufficient to produce it; the pupils contract, and intelligence again beams from his face; the tongue becomes a little moist, and in short, the whole of his symptoms change for the better; the swellings advance, in some cases rapidly, in others more slowly; he feels reduced, and is low, but complains of no pain, save that induced by the swellings gradually tending to suppuration or sloughing; he drags through a convalescence, which may be either short or long, complaining of extreme prostration of all nervous energy and power.

Such may be said to be the course of the disease in its severe form, when followed by a happy issue; but there are various modifications observed, a few of the most particular of which I shall now attempt to present. The primary action of the poison may be so very severe that immediate death may result. Diarrhœa and vomiting may be the first symptoms. These may exist singly or combined; when combined and very severe, they soon cut short the thread of life. The individual may fall into a low state, petechiæ appear, and death ensues before the period of reaction arrives; or, imperfect reaction may take place; he lingers for a few days as if struggling with the disease, and then—sinks exhausted. Such are the varieties worthy of notice in the first stage. In the second and third, reaction may be so severe as to induce furious delirium, with tossing or rolling of the head from side to side, coma supervene and death; or the looked for happy change may not take place at the fortieth or fiftieth hour, the skin retain its dryness, the powers of life yield, the extremities become cold, the breathing short and laboured, the *orbiculares palpebrarum* lose all power, the eyelids fall back, the eye becomes glazed, deadlike, and deeply sunk in the socket; the lower jaw falls down, petechiæ shew themselves on the arms and chest, the buboes either do not form, or if formed recede; involuntary evacuations take place, and he gradually sinks, quietly and placidly, or, on attempting some movement, suddenly expires; or swelling of the parotidian or submaxillary glands may take place quickly, producing suffocation;* or, lastly, intelligence may for a few moments return, he looks inquiringly about him, attempts to speak, an unmeaning placid smile curls around his lips, and then he closes his eye in death.

I have taken no notice of the state of the pulse in the different stages, because the existing regulations precluded the possibility of coming in actual contact with any diseased persons, without forfeiting for a time liberty. In one case only was I enabled to effect it, which case it is my intention to detail when speaking of the treatment.

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* Such was frequent at Sidon. See App. 7.

Description of Swellings, &c.—From a list which I prepared with much care and as much accuracy as the imperfect state of matters would admit, a Table, A, App. 5, has been compiled, presenting at one glance the various eruptions, singly or combined. Under the former, or simple, are comprehended buboes, carbuncles, gangrenous pustules, and petechiæ; whilst under the latter, or complicated, are placed the various combinations of the former.

Buboes.—The bubo, one of the very characteristic features of this disease, may occur in the axilla, the groin, the parotid region, and neck. As far as my observation and information extended,—I could learn no case of the popliteal or other glands, not enumerated, having been affected.

When the axilla or groin is to be the seat of this inflammatory action, the appearance is preindicated by twitchings, which take place late in the first day, or early in the second, extending in either case down the thighs or arms; a swelling soon becomes sensible; it may assume the appearance of an oblong tumour, situated in the centre of the axilla; or either above or below the crural arch, the former being the more frequent; or, the cellular tissue surrounding the glands may become affected with a diffused inflammation, giving rise to an extended tumour of a livid appearance, occupying more generally, when in the axilla, the inferior margin of the *latissimus dorsi*, or *pectoralis major*, and having the form of a pendulous bag; when in the groin, it may involve the whole of the triangular space included beneath the crural arch and the crossing of the Sartorius muscle. Sometimes there is a tumour situated above and below the crural arch,—that above following the course of the arch, whilst that below passes obliquely downwards. Lastly, they may occur so in only one groin, or both. When the neck or parotid region is to be the part affected, the pain is described as deep-seated; there is less twitching; the swelling advances with greater rapidity, gains a large size, and produces difficulty in both swallowing and breathing, in some cases causing death as by strangulation. The cervical suppurate freely, whilst the parotid can remain stationary; or if suppuration of the latter take place, it is merely superficial, the wound soon heals up, but an induration remains, which requires a long period to be removed. In some cases the inflammatory action is severe, as evinced by great pain and vivid redness of the cutaneous parts, tinged towards the centre by a livor, which varies in depth, colour, and extent; it is sometimes surmounted by a dirty-white gangrenous pustule, which, on breaking, shows a sloughing disposition of the subcutaneous cellular tissue. This last is called the *Pilot Bubo*, and is regarded as a very unfavourable prognosticator, very few who are thus affected recovering; it betokens a very debilitated or inactive state of the constitutional powers. The bubo, wherever situated, advances more slowly or more rapidly to suppuration—seven or eight days in the more favourable cases being regarded as sufficient. In some cases it advances to a certain extent, remains for some time stationary, and rapidly disappears with returning convalescence; in others it advances slowly, is attended with scarcely more than an irritative fever, and exists for fifteen or twenty days prior to bursting. The matter discharged is at first of a sanious nature, fœtid, and frequently mingled with blood, but gradually with the reinvigorating of the constitution, it assumes a more and more healthy appearance,—healing slowly takes place, leaving a livid colour of the integuments for some time, and a scar which, it is said, remains for ever. Many scars of many years' existence were shown to me as the scars of the plague buboes. Again, the whole subcutaneous cellular tissue appears destroyed; a sanious discharge is kept up; the aperture contracting, becomes fistulous, and the skin assumes livor more deep than that already described. The only cases in which this result was observed were debilitated individuals, who, in addition to an inherently bad and enfeebled constitution, could not in many cases obtain even the bare necessaries of life. From the appearances observed, and thus attempted to be described, it may be inferred, that the glands, although they may be the exciting cause, are not ultimately altered. The appearance conveys to the mind the conviction of the surrounding cellular tissue being principally, if not entirely, affected. Other swellings, which cannot be classed under either of the two foregoing heads, but which assume one of the appearances just described, take place on the breast, scapula, acromion, lower parts of the legs, &c. following the course of the bubo, they discharge a rather healthy pus, heal rapidly and kindly, but leave a lasting scar; their size, like their appearance, varies; they rarely, if ever, come on before the fourth or fifth day, but more generally during convalescence, resembling the boils which, under certain circumstances, are apt to take place during the convalescence from variola, &c. We find the buboes occurring alone or in combination with carbuncles, petechiæ, and gangrenous pustules.

Authors agree in stating that the safety of the patient very much depends upon the suppuration going on speedily and kindly. It appears, however, that those authors have overlooked the circumstance of the bubo being merely a symptom, and that the patient's safety does not depend on the rapid or kindly suppuration of the bubo, but that such occurring shows a good state of the constitutional powers, and may be taken as an index of the patient's favourable state or ultimate recovery.

Carbuncles.—The situations of this species of eruption may be the nates, the breast, the acromion, leg, face, neck, &c. They may, with great propriety, be described under two forms, which, although similar at commencement, differ materially in their course and results. The first indications of an approaching carbuncle are, a sense of itching, heat, and disagreeable sensation, referred to a small pimple, only discernible from its livid hue. It occurs early in the disease; and, after a few days, varying according to the intensity of the action which appears to be of a phlegmonous character, it assumes a conical form, the apex becomes livid, is generally surmounted by a vesicle of a dirty white colour, and surrounded by a blackish areola. After two or three days, it gradually increasing in size, assumes all the character of carbuncle, the inflammatory action appearing to be confined to the subcutaneous cellular tissue and lower stratum of *cutis vera;* the tumour soon becomes elevated,—it may be said to vary in size from that of a nut or walnut to the involving of the whole nates or neck. It is attended by severe stinging pain and great tension. The vesicle, becoming gradually larger, at length bursts, leaving an aperture with a blackened margin and base. A discoloured fœtid serosity first exudes, then an unhealthy suppuration is slowly established; the slough protrudes, and seems partly in that state, termed dissolved;—it gradually becomes expelled, leaving a large irregular cavity. Healthy suppuration, with an improving convalescence, takes place, granulations spring up, and, the part becoming filled, cicatrization takes place slowly, the newly formed cicatrix having a very dark blue appearance, which it retains for a considerable period. Subsequently, it is very apt to break out from the slightest cause. In the other form, the first stage is as already described; but, after a few days, from three to five, it takes on a very different appearance,—one so decisive and peculiar to itself, that, if once observed, it cannot be mistaken.

The whole substance of the *cutis vera*, the subjacent cellular tissue and superficial muscles appear to be involved. There is a sense of burning and contraction or binding of the part; the whole affected acquires a livid appearance; it gradually becomes more and more deep, and ultimately dies; the inflammatory action, of a low insidious creeping nature, killing as it goes, gradually extends itself; the *cutis* swells and thickens as if infiltrated, injected, or distended; gangrenous vesicles appear around the margin; fever of an ataxoid character takes place; and the vital powers, in many cases, seem so overpowered, that they have not energy sufficient left to form a line of separation. If the system rally, the line is gradually formed,—separation ensues,—the edges become turned up,—the whole part is thrown off, exposing to view a nicely executed dissection of the muscles and other parts subjacent to the sphere of its action, all being deprived of every particle of cellular tissue, subcutaneous or interfibral. With a return to convalescence, the granulations assume a more and more healthy appearance. Cicatrization takes place, and, according to the nature or situation of the part, is more or less succeeded by deformity or contraction. In one case attended, whose course will be detailed under the head of treatment, the whole cutaneous tissues from the lobe of the ear to the acromion process, extending around and below the clavicle and spine of scapula, were destroyed. Recovery in this instance took place, there existed considerable contraction and deformity of the neck, but not to such an extent as we had anticipated. They occur alone, or in combination with buboes, petechiæ, or with gangrenous pustules and petechiæ.

Petechiæ.—Their occurrence or appearance portend an almost invariably fatal result; although they are, in the generality of cases, confined to the thorax, lower part of the neck and arms, they are sometimes found to extend over the whole surface; they may occur separately or conglomerated. When in the latter form, they give rise to varied appearances, as lines, patches, &c. They occur alone, or in combination with buboes, carbuncles, or with carbuncles and gangrenous pustules.

Gangrenous Pustules.—They appear on various parts of the body; but, in the generality of cases, are confined to the extremities. They may appear late or early in the disease. A small portion of the skin becomes inflamed, assuming a circular form, rarely exceeding in size that of a pea. Its colour may vary considerably, but is generally a dark livid. After a short period, this patch becomes surmounted by a vesicle, which may be dirty white, livid black, or any of the intermediate shades. This vesicle appears to be an elevation of the cuticle alone, and its colour to depend on the nature and colour of the exhalation giving rise to it. They may occur alone, or variously combined; but their appearance, as already stated, indicates an adynamic state of the constitutional powers, and, if recovery do take place, it may always be expected to be protracted.

Various Complications.—The complications which occurred during the period which we are describing, may be understood by reference to Table *a*, App. 5, which will be found to be an analysis of 121 cases. In the table referred to are mentioned the varieties, the number attacked in each, with the results. From it, it will be observed, that out of 75 cases of bubo, of which number the situation of 17 is not recorded, there are 50 occurring in the groins, leaving only 13 for the axilla. In reference, again, to the groins, we find the left more frequently affected than the right;

but the mortality about equal. We have only one in the neck, and one in the parotid region.

Conclusions.—From the above, we may draw the following conclusions:—

1*st*, That inguinal buboes are the most frequent eruption observed, exceeding the axillary by a great proportion, and the carbuncles by a still greater.

2*d*, That, in relation to the groins, the left is more frequently affected than the right.

3*d*, That the mortality is nearly equal in the right and left,—the latter, however, having a preponderance.

4*th*, That the left axilla is more frequently affected than the right, and that there is a greater mortality in the left than right.

5*th*, That cases in which both groins are affected, or where we have two on the same side, are comparatively rare.

In relation to the complications, we find that the bubo and petechiæ are the most frequent,—that bubo, with carbuncle, holds a rank next, and scarcely inferior—whilst we have only one triple complication in the whole 121 cases—and that we have no complication of bubo, carbuncle, and petechiæ.

Divisions.—From the varied degrees of intensity exhibited by this disease, a division, with propriety, may be made into the three following: 1*st*, mild; 2*d*, severe; 3*d*, malignant. We shall now very briefly notice each variety.

1*st*, *Mild.*—Under this head, we may have the disease present itself in so simple a form, that the individual is scarcely so unwell as to be obliged to confine himself to the house, complaining only of "feeling a little unwell." We may have it occurring in a somewhat more severe form, ushered in by a sudden general uneasiness, and, though unattended by vomiting, with more or less epigastric tenderness. This state is in due time followed by a febrile reaction, which may be so mild as scarcely to attract attention; or it may be a little more severe, yielding to a gentle, kind perspiration about the commencement of the third day. This febrile reaction may be of a continued, remittent, or intermittent form.* The characteristic eruptions appear early, pass rapidly through their various stages, unattended with much pain, and heal kindly. Buboes or carbuncles may appear without any premonitory symptoms or precursory fever; but not unfrequently during their course does fever of a sympathetic or irritative nature supervene and continue, till the sores, the results of the enlargements, be nearly cicatrized.

2*d*, *The Severe.*—The description of the general features of the disease already presented is this, its more severe form, and, under this division, we find that we can class the great majority of the cases which occurred in the early part of the epidemic. It is always distressing in its course, and frequently fatal in its termination. In this form of the disease, the system appears to have power left to resist the evidently depressing powers of the "plague poison;" reaction, after a period, taking place, and assuming all imaginable shades or varieties, and yielding, if the termination be favourable, on the third day, to a kind perspiration, or take some other form, the *remittent* or *intermittent;* but whether in one or other of the two last, the patient will always be found in a state of extreme prostration, and with a better chance of recovery; or it may lapse into the typhoid or typho-adynamic state, or diarrhœa may supervene, and soon exhaust the little power left. In the early part of this form, vomiting, both frequent and distressing, is accompanied with great epigastric tenderness. There is a constant demand for cold fluids. The eruptions appear at their proper time, and go on kindly. It is in this form that that peculiarly diffused affection of the subcutaneous cellular tissue surrounding the glands is observed, and which we have described under the anatomical characters of the bubo. If an unfavourable change should take place after the second day, the buboes, &c. will be observed to recede. In the subsequent state, usually characterized by great prostration of physical power and exhaustion of nervous energy, care must be sedulously taken not to permit the patient to attempt any movement, as the suddenly raising the head may cause instant death.

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* We saw only one case in which this last form took place.

3d, The Malignant.—In this, the worst and most painfully distressing form of the disease, the patient is suddenly seized with a perfect depression of the whole powers of life, as if the poison introduced was so strong as to prevent entirely, or permit imperfectly, reaction from taking place. The patient is usually seized with vomiting, sudden loss of all power, oppressive dull headach, depression or rather indifference of mind, and confused intellect. The course of this, the first stage, may be so rapid, and at the same time so severe, that death may take place a few hours after the person has first felt unwell; diarrhœa, when it exists, accelerating the fatal termination. When reaction takes place, it will be found to be only a struggled one, the patient soon passing into a state of almost perfect insensibility, whilst from the disturbed state and low muttering, during what appears to be sleep, it is very evident to a beholder that very considerable internal distress exists. This state just described may continue without variation for a few hours; or the symptoms may become more and more aggravated, the evacuations pass involuntarily, lying upon his back, without the power of motion; his head falls on either shoulder; the mouth opens, the lower jaw hangs; the eyes turn towards the head; the breathing is long, deep, and laborious, the heaving of the chest considerable; the whole body bedewed with a cold moisture, oily-like, is described as cold to the touch; the buboes, &c., of a sluggish character, form imperfectly, pustules, vibices, or petechiæ take place, and after one or two ineffectual attempts to respire, death closes the scene.

During the prevalence of this epidemic it is no uncommon occurrence to find people suddenly seized without warning, fall down in the streets or in their houses, as if struck by apoplexy, and terminate life without a single struggle. Several occurred in Bairout, and everywhere they are regarded as a frequent and ordinary occurrence.

In the beginning of the season, and before several decided cases have occurred, it is difficult to distinguish plague from ordinary fever or bad remittent fever. The persons first attacked with what are called suspicious symptoms, especially if they come from suspected places, carefully conceal the attack; and it is only after cases become frequent and several persons have died that the alarm of the public and the authorities is excited, and then concealment can no longer be practised. When the disease has once developed itself, one who has once seen the peculiar expression of the countenance, the aspect of the tongue, the total deprivation of all power and energy, physical and mental, the shuffling unsteady step when supported, the quivering of the tongue when protruded, the exanthematous eruptions, need not hesitate. In the case of sudden death already spoken of, or where it may occur in its most simple form, we find ourselves surrounded with difficulties. The circumstances of its existence in the very town, the immediate neighbourhood, or in towns in the vicinity; the absence of the disease in either the town or district, but the person affected having very lately arrived from a town, village, district, or country where it existed; or the person thus under suspicion, being a stranger* from a healthy district or country, but arriving at that season in which the disease usually occurs as an epidemic, may, individually or conjointly occurring, at least justify us in pronouncing it, if not a true case, at least a suspicious one.

Statistics and Results experienced, &c.—In Table A, App. 4, an abstract is presented of 121 cases, which came under observation in Bairout or its immediate vicinity. By reference thereto, it will be observed, that a distinction is made between the civilians and military, the former being estimated at 18,000, the latter at

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* It is a fact as well established in this as in other epidemics and endemics, that individuals, who by residence have gradually become inured and habituated to the local influences, are much less likely to take the disease than strangers arriving in the district, even if from a very short distance; or if the disease exist in the locality they have just left, the absence of even a few days is quite sufficient to destroy this habituation, and make them susceptible of the impressions prevailing. The natives are aware of this fact, and gave me many cases illustrating it in the intermittent.

1800 souls. Amongst the civilians we have attacked 1 in $222\frac{6}{27}$, or 81 in the 18,000; whereas, amongst the military, we have 1 in 45, or 40 in the 1800. Amongst the civilians, again, we have a mortality of $34\frac{46}{81}$ per cent.; whilst amongst the military we find it to be $47\frac{1}{2}$ per cent. On the whole we find the following conclusions:—

First, That the civilians were attacked in less proportion than the military; and

Second, That the mortality amongst the military was greater than amongst the civilians.

Such results as those now stated were not confined to Bairout; for in Sidon the military also suffered much more. The following is extracted from the report in the Appendix, kindly presented to me by Dr Guillardot, M.D.P., Surgeon, &c. to the Turkish Military Hospital there. The population of Sidon, estimated at 5000 souls; the military then in garrison, 900 strong. Of the 5000 civilians, 2000 were seized, or 1 in $2\frac{1}{2}$, and 500 fell victims to it, or 1 in 4. Of the 900 military, 400 took the disease, and 190 fell victims to it, or 1 in $2\frac{1}{4}$.

At St Jean d'Acre, the attacks and mortality were still greater; but from the French medical officer there, and the English one at Jaffa, where it also committed ravages, both succumbing to the disease at an early period of its existence, we were unable to obtain reports on which reliance could be placed, or from which inferences might be drawn.

At the early part of the epidemic, the attacks were more sudden; the cases more severe, more numerous, more complicated, and attended by a greater mortality. By referring to Table C, App. 4, an idea may be formed of the number daily attacked, with results. It will then be observed, that, in the month of March, we have 10 cases, with 4 deaths; in April, we have 53 cases and 33 deaths; in May there are recorded 50 cases, with only 9 deaths; whilst in June we have presented 8 cases and only 1 death, or, in other words, we have the mortality as follows:—

In the month of March, .	1 in $2\frac{1}{2}$, or 2 in 5
April, .	1 in $1\frac{1}{2}$, or 2 in 3
May, .	1 in $5\frac{5}{9}$, or 2 in 11
June, .	1 in 8, or 2 in 16

Again, we have in the whole attacked 24 females and 97 males, with the following mortality:—

Females, . . .	1 in $4\frac{4}{5}$
Males, . . .	1 in $1\frac{13}{42}$
And in the whole attacked, .	1 in $2\frac{27}{47}$

From which we observe, that the females less subject to the disease, do, when attacked, less frequently fall victims to it. This last fact may partly perhaps be accounted for by the perfect seclusion which the female part of the community are made to observe in the East.

On the 24th of June, St John's Day, a day hailed with universal joy and rejoicing, the inhabitants regain confidence; all private quarantine is abolished, the inhabitants no longer believing the disease capable of existing, or if it did, no longer endowed with contagious properties.

Cause, Nature, and Contagious Properties of the Disease.—Plague prevails in certain countries only. It occurs at stated periods only as an epidemic; it follows a more or less regular course; it is influenced by temperature; and in Syria it disappears from the plains with the heat of summer, and from the mountains by the cold of November or December. It is regarded by the superstitious and credulous inhabitants of the East as produced by a very small insect, which, alighting on the body and entering the circulation, excites fever, and causes by its escape the characteristic swellings to appear. They moreover affirm, that it is not permitted all to see this insect, but that only one or two of their more favoured wise men have observed it. They* consider the disease to be endowed with very highly contagious properties, to be generally imported into their country from Egypt, and to be propagated by contact alone. So convinced are they of the last mentioned circumstance being the case, that, from the moment of its appearance amongst them, they separate themselves, and avoiding contact with any save their own families, carefully follow those various observances, to be treated of presently under the head of prophylactic means; moreover, of so very highly contagious a nature do they credit it to be, that they fully believe that a person having had contact with an affected person, or some one who may have had contact or intercourse, may, although not suffering from the effects of that contact, or not labouring under the disease, give it to another, and propagate it in every direction. As far as our investigations went, we believe it to be possessed of contagious powers, which may, according to concomitant or adjuvant circumstances, exert a more or less extended sphere of action; but that it did not, in Bairout, exist with those properties in so powerful a degree, as it usually is represented to possess, I think is manifest from various facts. The following, a few only of the facts in our possession in corroboration of the above advanced opinions and assertions, may be adduced.

1. Six persons believed to be infected, with the servant of his Excellency, Omar Pasha, were confined for the first few days in the same apartment with him. Thereafter they were removed to a room immediately adjoining, but having a door of communication. After twenty-five days, on being examined, they were found well, and admitted to pratique.

2. Thirty-seven persons fell under suspicion of being infected

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* The inhabitants of Syria.

by having been in the same room with a soldier who died of the disease; they were all removed to the Lazaretto, and, after thirty-one days, were admitted to pratique, none having taken the disease; whereas,

3. Four guardians, who had been employed in transporting the said soldier from the Khan at the Marina, to the Lazaretto, on their return, were shut up in an isolated fort, to undergo a certain prescribed quarantine. On the ninth day thereafter, two took the disease; the day following the others were found to have been affected also. Of these four two died, one on the fourth day after being attacked, the other on the sixth.

4. Thirty-one persons fell under suspicion of being infected with the cavass (servant) of the Seraskier Pasha; none took the disease, and after a certain quarantine were discharged "all healthy."

5. Ten persons arrived per bombard from Alexandria, and were put into the Lazaretto, with a guardian, on the 29th of March. On the 2d of April, a girl, aged 7, after vomiting severely, expired in six hours after the first complaint. On the 4th, her sister was seized in a similar way; a carbuncle appeared on the left inferior palpebra, petechiæ supervened, and late on the second day she expired. The father of these two girls performed the *spoglio*,* and with the rest of the passengers, none of whom had had a complaint, took pratique on the 20th of April. He retired to his house in the gardens, and put himself and family into strict private quarantine. On the first of May he was reported dead and his slave ill; on inspection, his corpse presented a bubo on the left groin in an advanced state. His slave also died. The disease did not extend farther through the family.

6. Eighteen houses were inspected, in which plague had existed and did then exist. In these 18 houses were found 85 inhabitants, and amongst them 28 cases of plague. Of the 28 cases, 15 recovered, 13 died; and of the remaining 57, who, in the generality of instances, lived in the same apartments, attended, and nursed the sufferers, not one took the disease.—Vide Tab. *b*, App. 4.

7. In the case of Antoon, whom we attended, his mother and two servants, who constantly administered to his wants and necessities, slept in the same apartment, had their hands oft imbued in his secretions, and washed the cloths for the cataplasms; yet, for months afterwards, none had the slightest illness. (N. B.—The room was large, which was kept constantly ventilated, and fumigated from time to time by vinegar and chloride of lime.)

8. Of the guardians attending at the Lazaretto, fourteen took the disease and eight died; the number actually attending we never could ascertain.

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* A term used to denote certain forms through which a compromised individual has to pass, so as to be enabled to mix with society without undergoing quarantine.

9th, At the Lazaretto, from want of properly cleansing the apartments appropriated for the plague patients, the effluvium issuing from the rooms was so heavy and offensive, that it could be perceived at some distance. All the guardians employed took the disease, and the very soldiers on guard around the building were attacked. The latter were at last compelled to be removed altogether. This will not excite astonishment, when it is stated that in these rooms, the dead were allowed to remain with the living sufferers till the medical inspection in the morning.

10th, On the 24th of May, whilst the disease still existed, but not to so great an extent, or in so severe a form, nor yet attended with such mortality, the Turks, who had long, with patience, submitted to so great an infringement on their religious feelings, the destroying with lime the bodies at the Lazaretto, at last rebelled against the authorities, and taking possession of the body of the secretary to the Mousta Shaw Effendi (Commissary-General), raised it in their arms, formed a procession, entered the town, and carried it in triumph to the Mosque, where, being met by many more, and being joined by the guard of soldiers, who were sent to restrain them, they rubbed their hands and faces with the clothes torn from the body, then washed it according to their rites, &c., and many, in the height of their infatuation, knelt down and kissed it. Lastly, decorating it with evergreens, they carried it amidst a procession composed of hundreds, and interred it in their own burying-ground, immediately outside the town. Inquiries instituted with care in every quarter, and continued for weeks, failed to detect any extension of the disease from this circumstance.

Many others might yet be adduced; but the foregoing we deem sufficient to show, that, although the plague is a disease evidently possessed of contagious properties, and which, for want of proper care and cleanliness, may become increased; yet in Bairout it did not prove itself to be of that contagious nature which it is represented and generally believed to be. But we remark, that so long as we have to deal with those local influences which engender the disease, and always operating, tend to propagate it, we must expect to meet, at every step, results as contradictory as those we have now related. Referring, in the first instance, more particularly to No. 2 and 3, where we have 37 soldiers in the same barrack room, with one of their companions not seen till life had fled, and in whom the evidence was such as to show clearly, that the disease must have existed for several days, yet not one individual takes the disease, although in actual contact with him and one with the other; whereas we have four men who merely transport the lifeless body to the Lazaretto, where the last offices were performed, reconducted to town, placed in an isolated fort, and the whole take the disease, two of the number dying. Again, we found four cases in a Turkish hospital, one already gone, and a second, who seemed in the agony of death, and was making use of all that remained of his late companion for a pillow, died shortly afterwards. The other two soon followed, and the disease extended to every person in and connected with the hospital. *Again*, we have detailed in No. 6 the inspection of 18 houses, in which we may properly be permitted to assert, that the contagious power did not demonstrate itself, whilst, in the apartments of the Lazaretto, from causes already mentioned, we have generated a focus of contagion, of so very powerful an influence, that it even cuts down the guards around the building. Moreover, we find everywhere numerous instances in which it sometimes cuts off every member of a family, or one solitary case only presenting itself, and not extending farther. We have some towns entirely or almost depopulated, which, from their situation and other contingent circumstances, we would say, could barely have encouraged such an idea, while others again, whose situation, &c. would seem to invite it, scarcely or not at all suffer. We find, in fine, this disease following the same unaccountable course, and observing much the same peculiarities, &c. as other epidemics present.

We have spoken hitherto *only* of the disease where it has shown itself under the existence of local influences, &c. We would now look to the other side, or its occurrence in a place where either the local influences do not prevail, the disease having been imported, or in such a situation, that, in a few hours, we can avoid or escape from them. We have it in our power to present cases illustrative of both those circumstances; the first establishing two points, viz. the production of the disease by contagion, and the arresting of the disease by the use of proper means; the second, showing the deprivation of contagious property or power, and the arresting of the disease by a rapid change of situation, &c.

1*st Case.*—A person arrived at Constantinople from the southward: after a few days he complained, and was, on inspection, found to labour under plague,—a disease which did not then exist, and had not, for many years previously, existed at Constantinople or in its neighbourhood, but which was raging in the district at the time the man left. His sole attendant took the disease, but it did not farther extend. *2d*, The Castor frigate, whilst in Acre Bay, was unfortunate enough to contract the disease,—the symptoms appearing in the persons of some sailors whom they had embarked, and who had been employed for some time previously at the fortress. Thirteen took the disease; the frigate immediately stood off for Malta. The disease, we believe we are correct in stating, only extended to those individuals who had been employed on shore, and neither the medical officers or attendants, although in continual communication with, and attendance on, the sick, took the disease. Were it necessary still farther to enter into this portion of the subject, we could adduce from record many proofs tending to show with how much ease and certainty the disease may be controlled, when it does not exist in the place or situation as an endemic. Those who feel interested, we would refer to that elaborate repository of medical literature, Mason Good's Practice of Physic.

(To be continued.)

ART. V.—*On Inflammation as a process of Anormal Nutrition.* By JOHN HUGHES BENNETT, M.D., F.R.S.E., Fellow of the Royal College of Physicians, Lecturer on Pathology and the Practice of Physic, Pathologist to the Royal Infirmary, Edinburgh, &c. (Concluded from No. 158, p. 54.)

VI. TERMINATIONS OR RESULTS OF INFLAMMATION.

THE expression, termination of inflammation, which has been sanctioned by general usage, is here used to signify the changes which the *liquor sanguinis*[*] undergoes after having been exuded through the blood-vessels. We have described the early phenomena, and the essential phenomenon of inflammation, and these changes constitute the resulting or subsequent phenomena.

What have hitherto been considered as the terminations of inflammation, viz. adhesion, softening, induration, suppuration, granulation, ulceration, gangrene, &c., modern pathology has shown to be in themselves highly complicated processes. John Hunter was of opinion that these various results were explained by supposing the inflammation to undergo certain modifications, or that it terminated differently according to certain tendencies which it possessed; hence the terms adhesive, suppurative, ulcerative and gangrenous inflammation. According to the view, however, which we have endeavoured to establish, viz. that inflammation essentially consists in an increased exudation of blood-plasma, it follows that this process in every tissue and under all circumstances is the same. The exudation having once taken place, any further changes we may observe in it will depend upon the amount or rapidity with which it is poured out, the tissue in which it occurs, its chemical and vital proportions, and the accidental circumstances which modify or destroy growth both in the vegetable and animal world.

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[*] It is perhaps necessary to say that we employ the terms *liquor sanguinis* and blood-plasma as synonymous with each other, understanding by them the fluid portion of the blood composed of fibrin dissolved in serum.

Whenever the exudation of blood-plasma poured out coagulates, it can only be removed from or assimilated to the economy by one of two results, viz. by *death*, or by passing into *organization*. When it *does not coagulate, which is an occurrence of great rarity, it may again pass into the vessels by endosmosis*, and in this way the parts return to a normal state. *Resolution*, however, as at present understood, is a very compound process. It is used to express the disappearance of the inflammation *without its producing* any external lesion. In this manner resolution may follow hæmorrhage, softening, or suppuration, and great difficulties impede our attempts to ascertain with exactitude how, under these circumstances, the exuded and organized blood-plasma is absorbed. For this reason, therefore, we shall discuss this portion of our subject in the following order: 1st, the termination of inflammation in death of the part; 2d, the termination in organization; and 3d, the termination in resolution.

1. *The termination of inflammation in death of the part.*

This may be either acute or chronic, the former constituting mortification or moist gangrene, the latter ulceration.

Mortification.—Occasionally a very large amount of blood-plasma is thrown out, constituting a violent inflammation; a greater or less number of capillaries are also ruptured, and blood corpuscles are more or less mixed up with the *liquor sanguinis* exuded. The exudation thus formed compresses the part so as to obstruct the blood-vessels, and prevent the continuance of any circulation in it. Under these circumstances, instead of forming a blastema for the production of new organisms, it undergoes chemical changes which induce in it decomposition, and the part is said to be mortified, or to be affected with moist gangrene. This change commences first in the blood extravasated, which becomes of a purple colour, more or less deep; the corpuscles break down and become disintegrated, their hematozine dissolves and colours the serum, and should the exudation have coagulated, it forms brown, rust-coloured, purple, or blackish masses. An acid matter is now formed, which, acting on the neighbouring tissues, produces fœtid gases, that are abundantly given off from the affected part. Sulphuretted hydrogen is evolved, which causes the blackish sloughs usually observed in such cases, and discolours silver probes and the preparations of lead. After a time, the elementary tissues surrounding or involved in the exudation become more or less affected. The transverse striæ in the fasciculi of voluntary muscles become first pale, and are then obliterated. Cellular tissue, fat, and other soft substances, lose their connection and fall into an undefined granular mass. The tendons and fibrous tissue retain their characteristic structure for a long time after the other soft parts have been reduced to a softened pulp. The bones resist the action longest, but at length become rough, soft, and, commencing externally, are more and more broken down, and reduced to the same pulpy consistence and granular structure as the surrounding parts.

As the tissues thus become broken down and fluid, they are discharged from the system in the form of an ichorous matter, which, examined microscopically, presents numerous granules, imperfect or broken down cells, blood corpuscles, and fragments of filamentous tissue or the other structures involved. If the morbid action be seated in the subcutaneous tissue, the skin soon becomes involved, and an opening is formed, which rapidly increases and gives vent to the discharge. In a similar manner gangrene of internal organs, by destroying the intermediate parts, at length enables the discharge to reach the surface, or to find its way into the excretory passages, such as the bronchi, the intestinal canal, the *meatus auditorius*, &c. In this manner life may be endangered by the destruction of organs important to life, by the exhaustion occasioned by the discharge, and sometimes by the absorption of the ichorous matter, which, by entering into the circulation, acts as a poison to the economy.

It may be asked whether inflammation and gangrene are similar processes,—whether the latter is only a greater intensity of the former,—or whether, when gangrene follows inflammation, it is dependent on other circumstances, such as a peculiar state of the atmosphere favouring the decomposition of the exudation poured out. In order to answer any of these questions we must distinguish between mortification arising from a variety of circumstances, and an inflammatory gangrene properly so called, the which is undoubtedly the rarest of all the terminations of inflammation. We frequently see mortification produced by the application of chemical or mechanical agents, which directly destroy the tissues. It also arises from severe and complicated injuries, in which arteries leading to the portions of structure affected have been divided or crushed. In old persons it follows obstruction in the blood-vessels, or is dependent on circumstances not yet ascertained. In none of these cases is it caused by inflammation. But when stasis of the capillaries is produced to a considerable extent, followed by the exudation of a large quantity of blood-plasma, which, instead of passing into organization, undergoes the changes previously described, then an inflammatory mortification, properly so called, is produced. We see this take place after burns, a long exposure to frost, and in certain cases of erysipelas. Here the amount of exudation is considerable, the pressure caused by it extreme, the obstruction to the circulation in the neighbouring parts correspondingly great, and these, as well as the exudation itself, die. In this sense, therefore, it may be said to depend on the severity of the inflammation. This, however, is not the case in the sense of those who consider the adhesive, suppurative, and gangrenous inflammations as different stages of one process. Suppuration, as we shall afterwards see, has no connection with adhesion, nor is it in any way related to mortification. It is a primary alteration of the exudation. Its vitality is lost, and, instead of passing into organization, as we shall hereafter describe it, it at once becomes subject to the chemical laws of dead matter.

On the other hand, mortification occasionally arises under circumstances in no way connected with the amount of the exudation, or the rapidity with which it is thrown out. During the summer of 1836, I watched with great care the progress of a sloughing gangrene, prevalent not only in the Infirmary of Edinburgh, but through the city generally. All kinds of sores and wounds were affected by it, even those of a specific nature, such as chancres. Neither youth or age was exempted from it. It affected not only those who were debilitated from disease, by intemperance or bad diet, but those also in the most robust health. Thus a servant girl, aged 16, who had never suffered from illness, and of a robust constitution, fell down upon some glass bottles, and slightly cut her left thumb. A week after, she entered the Infirmary with an ulcer the size of a shilling, filled with a brownish-black slough, and discharging a fœtid and sanguineous fluid. In this, as well as other cases which occurred, it became impossible to attribute the gangrene either to the violence of the injury, the amount of exudation, a state of cachexia, or indeed to any circumstances connected with the individual. It could not arise from contagion, as it existed in different parts of the city, was not confined to the Infirmary, and the system of dressing wounds there precludes the possibility of this explanation. We are, therefore, compelled to ascribe the cause to something without. Most writers have noticed the connection between a certain state of the atmosphere and the prevalence of hospital gangrene.* Its frequent occurrence in summer, that is, at a period of the year when increased temperature favours the decomposition of animal matter, and the good effects which result from change of air, when every kind of treatment fails, still farther point out its origin from changes occurring in the atmosphere.

Ulceration.—The process of ulceration is somewhat similar to that of mortification, only it is more chronic, and the exudation, instead of undergoing decomposition, only exhibits an indisposition to pass into organization. In this case the exudation is poured out slowly, it coagulates and presses upon the surrounding parts, more or less obstructing the flow of blood to them, and acts as a foreign body. By means of the continued pressure, the circulation is obstructed, and death of the portion affected results. Sometimes this is imprisoned in fresh exudation, as ulceration extends, and the whole at length becomes disintegrated. All this time the exudation exhibits little of that tendency so conspicuous in healthy persons to undergo changes in itself, and when examined microscopically is found to consist principally of very minute granules, varying in size from $\frac{1}{15.000}$ to the $\frac{1}{500}$th of a millimeter in diameter. These are occasionally mixed with irregularly formed cells, usually more or less angular, containing one or more granules. The cells are more numerous in proportion to the stage of the ulceration, and the healthy powers of the constitution. These different granules and imperfect cells, with the structures they involve, at length become broken down, and separated from each other, constituting a semi-fluid mass, which has a tendency to point where it can most readily be discharged, that is, towards the surface of the skin or mucous membranes. Here, on account of the less degree of resistance offered, the continued pressure and disintegration of tissue first causes an aperture to be formed. Another portion of solid exudation is now broken down, with the tissues involved in it, and in this way the opening is enlarged. If the morbid process continue, a fresh exudation is slowly poured out below the already coagulated blood-plasma, which supplies the loss thrown off in the form of discharge, and thus chronic ulcers may be continued indefinitely. The whole of this process may be well observed in scrofulous and syphilitic ulcers, or in the callous sores of the leg in weavers and others of a cachectic constitution. Indeed the general powers of the constitution are almost always in such cases enfeebled, and hence the indisposition of the exudation to be transformed into organized cells.

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* See Boggie, Thomson, Hennen, and Blackadder on Hospital Gangrene.

Ulcers produced by direct pressure are occasioned in a similar manner; only in such cases the pressure is not derived in the first instance from the solid exudation poured out. Thus, in stumps not sufficiently covered by soft parts, in places long pressed upon by lying, or by the growth of tumours, the vitality of the part is slowly destroyed. At the same time an exudation is poured out from the neighbouring vessels, which becomes broken up, and assists in disintegrating the textures whose vitality is destroyed. The finely molecular particles are thus absorbed, whilst the grosser portions are thrown off in the form of discharge.

All ulcerated surfaces are covered with a fluid, which varies in character according to the nature of the sore. Healthy granulations are covered with laudable purulent matter, and the corpuscles present their normal character. In chronic scrofulous and syphilitic sores the corpuscles are generally of an irregular form, constituting what has been denominated anormal purulent matter. Not unfrequently the ulcer is covered with an unhealthy-looking discharge, either of a thin dirty yellowish tint, or more or less sanguinolent and fœtid. In the latter case the discharge has received the name of sanies, and is similar in character and constitution to that observed in the fluid accompanying inflammatory mortification. That is to say, there are traces of imperfect cell formation, mixed with numerous molecules, and the shreds or debris of the structures involved.

Ulceration has by most writers since the time of Hunter been regarded as the result of a peculiar operation, which he denominated ulcerative absorption. No doubt the process, such as we have described it, is peculiarly favourable to the production of a fluid containing molecules so minute that they may readily permeate the neighbouring vessels by endosmosis. On this subject we shall dwell more at length under the head of resolution or absorption. In the mean time it must not be overlooked that much of the loss of substance observed in inflammation, especially of the more consistent and tough structures, after having been more or less broken up, are thrown off from the surface in the form of discharge. This is proved by direct observation. In either case all such parts first loose their vitality, from the pressure to which they are subjected, and then being broken down, the fluid and finer parts may be absorbed, whilst the coarser are thrown off from the surface.

The treatment in all such cases must evidently be to remove the pressure which destroys the vitality of the part. When mechanical causes exist the mode of doing this becomes obvious. But when, as in the case of constitutional ulcers, the pressure is derived from an exudation which has no tendency to organization, then our efforts must be directed, 1*st,* to cure the constitutional disease, and thereby change the morbid condition of the blood-plasma; *2dly*, to facilitate the disintegration and discharge of the coagulated exudation by means of alkaline lotions; or, *3dly*, to favour its passage into organization by fomentations, poultices, &c.

When loss of substance occurs by accident, or from wounds in a healthy constitution, there is no process of ulceration properly speaking. Nature immediately commences to repair the injury, and completes this by the process of granulation and cicatrization. These results will more properly be considered under our next head, namely,

2. *The termination of inflammation in organization.*

From what has previously been said when speaking of the cellular theory of nutrition, it follows that organization consists in the formation and development of nucleated cells. Thus, purulent matter is not to be considered as a secretion from the blood, to be like urine excreted from the body, but as a fluid in which, like the blood itself, numerous corpuscles or organisms float. These corpuscles have a distinct structure, they each possess vital power independent of each other, and pass through a definite origin, growth, and decay, like all other living beings. It is in this sense, therefore, that we employ the term organization, without reference to the existence of vascularity, which for so long has been considered the great characteristic of new vital formations.

In the great majority of cases, when inflammation has occurred, the blood-plasma exuded seems as a blastema, in which numerous cells are produced and nourished. These cells are of various kinds, and possess different powers of development. Some complete their growth when the cell-wall has reached its utmost size. In others, numerous granules are formed; whilst a third class undergo a gradual metamorphosis into more compound tissues. These either resemble those structures which have been inflamed, unite surfaces which have undergone a breach of continuity, and serve as a substitute for any part which has been removed, or, in particular states of the constitution, they form the various kinds of morbid depositions and growths, either simple or malignant. The circumstances which induce or influence these different results, will be treated of in a separate section; in the meantime, it may be said that the exudation becomes organized as follows:—1*st*, By the formation of (*plastic*) corpuscles and primitive filaments, constituting lymph; 2*d*, By the formation and disintegration of (*exudation*) corpuscles, producing softening; 3*d*, By the formation of (*pus*) corpuscles, occasioning suppuration; and, 4*th*, By the formation of corpuscles which pass into filamentous, fibrous, osseous, muscular, or other tissues.* One or more of these processes, as we shall afterwards see, may be combined together; but it will be necessary for us, in the first instance, to treat of each separately.

1. *Of the transformation of the exudation into primitive filaments and corpuscles, constituting lymph, as seen on the surface of serous membranes.*—Here the blood-plasma exuded coagulates, the fibrinous portions solidify and attach themselves to the membrane, whilst the serum is set free and occupies the intervening space of the shut cavity. In this case, when the inflam-

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* To complete the history of the exudation, it would be necessary to add, 5*th*, The formation of granules and imperfect cells, constituting tubercle; and, 6*th*, The formation of variously formed corpuscles and filaments, which, in the aggregate, constitute tumours, either homologous or hetorologous. We shall treat of these on another occasion.

mation is acute, the process is identical with the coagulation of the blood without the presence of blood corpuscles. The coagulated portion is now usually termed lymph, and presents different appearances according to the rapidity with which it is thrown out, and the extent of the membrane affected.

In very acute cases of pleuritis, which have proved rapidly fatal, I have repeatedly observed the following appearances. On carefully elevating the sternum, care having been taken not to disturb the body for some hours, the pleural cavity on the side affected has been found full of apparently a clear fluid of a yellowish or greenish tint. On emptying this by means of a small shallow cup, the first portions removed have been perfectly clear and transparent. On continuing to empty out the fluid, it has been observed that the deeper we descend the more turbid it becomes, until at length nothing but a semifluid mass is removed. It will frequently be found that large portions of this mass, although tolerably consistent, are semitransparent, resembling a light-coloured calf's-foot jelly, whilst other portions present the usual opaque appearance. Sometimes, when the body has been undisturbed for twenty-four hours, the whole exudation is separated into two distinct portions. The upper, fluid and perfectly transparent, whilst the lower is composed of a pultaceous mass, resembling a bread-and-water or oat-meal poultice. In all such cases, the fibrinous portions, from their superior specific gravity, have sunk to the bottom, whilst the supernatant serum remains clear. The semitransparent lymph is the portion most recently exuded, in which very few of the plastic corpuscles, afterward to be described, have been developed.

When the progress of the exudation is less rapid, the lymph assumes a more consistent appearance, and forms flocculi of different sizes, or a distinct lining, varying in thickness from half a line to an inch, over the part inflamed. Occasionally it is smooth, but sometimes it presents a honey-combed appearance, or hangs in loose flakes of a dendritic appearance in the serum. The surface is generally smooth on lymph effused in the subarachnoid cavity. It is frequently honey-combed on the pericardium, which is owing to the repeated closing and separation of opposed surfaces. It is often dendritic or stalactitic on the pleura and peritoneum, and is probably owing to one drop of plasma being exuded on a portion which has previously coagulated, as Vogel suggested, in the same manner that stalactites of carbonate of lime are formed in a cavern. Serous membranes, when inflamed, frequently resemble mucous surfaces, from the villous appearance the effused lymph assumes; and, in point of fact, it performs the functions of a mucous membrane for a time, and is very active in absorbing the serum, which, if the inflammation ceases, gradually disappears. This villous aspect of lymph is very commonly seen on the pericardium and peritoneum. Occasionally also it assumes a lamellar arrangement, attributable probably to repeated exudations of blood-plasma at different times. This may be frequently observed in the pleura, and layer after layer readily dissected off.

When the inflammation has been less acute, or is of longer standing, we find, after death, that the coagulated blood-plasma or lymph has become more consistent. It never assumes a fibrous appearance, often extending between opposed serous surfaces in the form of bands, which have considerable tenacity and strength. These bands have a great disposition to contract, and ultimately become shorter and shorter, and assist in forming a dense substance, which at length firmly unites together the serous surfaces. This uniting substance becomes more and more dense, and not unfrequently resembles ligament in toughness and general aspect. In this form it may frequently be seen in phthisical cases, uniting together the lobes of the lung. Occasionally it assumes even a cartilaginous hardness, resembling the fibro-cartilage of the intervertebral surface. In this state it may frequently be seen on the spleen, and I have seen it thus half an inch thick, intimately uniting the lungs to the ribs. Lastly, when it has been very slowly developed, it produces white indurated patches, of a glistening cartilaginous appearance, varying in extent, the surface of which has assumed the character of a serous membrane, and in no way interferes with the movements of neighbouring organs. Such patches are exceedingly common on the surface of parenchymatous organs, as the heart, liver, spleen, and kidneys.

On examining the minute structure of lymph, in the various forms and stages now described, we find the following: At the very earliest period, when it presents the gelatinous semitransparent appearance, we find it to be made up of a minute filamentous structure, first described and figured by Gruby,* and subsequently by Gulliver† and Vogel.‡ It is identical with the structure observed in the colourless clots of blood so frequently found in the heart, and with the buffy coat of inflamed blood, as first seen to form under the microscope by Addison.§ The filaments appear to be made up of exceedingly minute molecules, arranged in rows, similar to what is seen in mucus on the addition of acetic acid, (Plate VII. Fig. 2.) When recent lymph becomes opaque, or, as it is generally observed, it consists of two elements; 1*st*, The same filaments just alluded to, which, however, may now be observed to be larger in size, more distinct, and less molecular; 2*d*, A quantity of corpuscles, which are entangled among the filaments, although easily separated from them by washing, or by pressure between glasses, (Fig. 3.) These corpuscles, for the most part, are from $\frac{1}{100}$ to $\frac{1}{75}$ of a millimetre in diameter, formed of a delicate wall, containing granules from $\frac{1}{1000}$ to $\frac{1}{500}$ of a millimetre in diameter, which vary in number from 3 to 12. They are not perfectly round, but somewhat irregular in form. They differ widely from purulent matter, for which they are usually mistaken, not only in their general appearance, (which will at once be recognised by the experienced observer,) but in the effect produced on them by reagents. Thus, on the addition of acetic acid, they frequently undergo no change, or become more transparent, and at others the wall contracts, and the edge becomes thicker and more irregular. It is dissolved in ether and caustic potash, and not affected by water. These corpuscles have been described and figured by Gruby* and Vogel,† as those of purulent matter, by Valentin‡ and Gulliver§ they have been denominated exudation corpuscles. We shall see, however, that both these terms have been applied to other structures. With a view of distinguishing them, therefore, from these, I denominate them *plastic corpuscles,* on account of their constituting the characteristic structure of plastic lymph.

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* Obs. Microscopicæ. † Gerber's Anatomy.

‡ Icones Histologiæ Pathologicæ.

§ Prov. Med. and Surg. Associat. Trans. Vol. xi.

All specimens of recently exuded lymph on the surface of serous membranes will be found to be made up of the filaments and corpuscles now described. The former are most abundant in those portions which adhere together, and the latter in the gelatinous, pulpy, and fluid portions. The corpuscles found in all recent inflammatory effusions occurring in the pleura, pericardium, and peritoneum, will be found very rarely to contain purulent matter, but most commonly an innumerable number of plastic corpuscles, occasionally mixed with a few filaments more or less broken up.‖

At a later period, when most of the fluid has been absorbed, and the lymph is more consistent, it will not unfrequently be observed, that cells exist in it which are undergoing development into fibres, as noticed by Henle¶ and Engel,** (Figs. 5, 6, 7.) Mixed with these will be observed, filaments in every respect similar to those observed in the ordinary cellular tissue of descriptive anatomists. In more chronic cases, where the exudation resembles bands, the structure is identical with that of filamentous tissue, and when it firmly unites opposite surfaces, the minute fibres are arranged close together, as is observed in the fibrous or ligamentous tissues in general. The white, glistening, and indurated patches so frequently seen on the spleen and heart, do not present a cellular structure, but are densely fibrous, and analogous to fibro-cartilage.

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* Obs. Microscop. Fig. 64.

† Tab. Histologiæ Path. Pl. 3, fig. 5.

‡ Repertorium, 1838, p. 273.

§ Phil. Mag. Vol. xxi. p. 171.

‖ See a valuable paper on this subject, by Mr Gulliver, Phil. Mag. Vol. xxi. p. 241.

¶ Bericht. p. 171.

** Oesterr. Med. Wochen. 1843, No. 3.

Such are the ordinary appearances and minute structures which the exudation presents when poured upon the surface of serous membranes. The filaments in recent lymph are evidently the result of deposition, which is produced as the fibrin of the *liquor sanguinis* coagulates. These are primary. But are the filaments in old lymph the same, only increased in size, or are they the result of cellular development, that is to say, secondary? From the occasional appearance of cells undergoing the transformation into fibres, as originally described by Schwann, this seems probable. At least it is certain that secondary filaments are developed amongst the primary, which communicate to the tissue increased strength and elasticity, and bestow upon it that highly contractile property so characteristic of effused lymph. After a time, we can observe no traces of the primary filaments, only fibrous tissue remains. Whether they are dissolved, or in what manner they disappear, I have not yet been enabled to determine.

In this, as in all other kinds of blastema, we find corpuscles produced. We have shown that they are not those of purulent matter, and have characters which distinguish them from other cell-formations. It may be asked, how are they formed, and what is their ultimate destination? On these points our knowledge is in a very unsatisfactory state. They only contain granules, without any distinct nucleus, and hitherto we have been unable to trace their origin. Again, whether their life terminates with the degree of development which has been described, and they are again dissolved; whether they are the nuclei of the fibres afterwards produced, or whether they are ever transformed into purulent matter, are points equally unknown. Some facts, to which I shall subsequently allude, indicate that corpuscles analogous to purulent matter are occasionally produced even on serous surfaces. It may also be asked, are the cells which undergo transformation into fibres a subsequent production? Do they owe their existence to an after exudation from new vessels which have been formed? All these queries can only be solved by further investigations. It is at least certain, that the changes observed in chronic lymph, the transformation into filamentous and fibrous tissue, is attributable to the development which cells undergo, according to the well-known law of cytogenesis, as brought forward by Schleiden and Schwann.

2. *Of the transformation of the exudation into granules, masses, and cells, and the occurrence thereby of inflammatory softening, as seen in the substance of parenchymatous organs.* —In parenchymatous organs, such as the brain and lungs, we find

that the exudation of *liquor sanguinis* is followed by a very different appearance from what characterizes it when poured out on the surface of serous membranes. Here, instead of being spread over a surface and coagulating in a continuous sheet, it is insinuated among the elementary structures of the organ, into every minute space, imprisoning them, so to speak, in a cement which afterwards solidifies. At the same time, a great portion of the serum is rapidly absorbed. In the generality of cases, a quantity of blood is also extravasated with the plasma, more especially in the lungs, where this lesion is denominated red hepatization. With respect to this organ, indeed, it would seem at first sight to be hardened by this change, and, in point of fact, it becomes more dense, and readily sinks in water. Reflection, however, will show that the term hardening here is merely comparative. A lung so affected is undoubtedly harder and heavier than it was in its previously spongy, light, and healthy state, on account of the displacement of air by the solidified exudation. But if the finger be thrust into a healthy, and then into a hepatized lung, it will be found that, whilst the former offers considerable resistance, the latter is friable and readily breaks down, proving that, in point of fact, an inflammatory softening of the tissue has taken place. In the brain, liver, or any other organ naturally dense, the softening from inflammation is rendered at once apparent, and, when increased, is found to be pultaceous and even diffluent. In the brain they assume various colours, and are called the red, fawn, yellow, and white softenings, which depend upon the amount and condition of the blood effused, and the greater or less admixture of serum. In the lungs the softening may be red or gray, but this is dependent upon the formation of different organisms in the part affected.

The minute structure of the exudation in parenchymatous organs varies according to its amount and the rapidity with which it is poured out. The exudation, instead of coagulating in the form of filaments, as when lymph is produced, consists of minute granules, varying from a size scarcely measurable up to the $\frac{1}{500}$th of a millimetre in diameter. This granular formation may be seen coating the vessels, in layers more or less thick according to the quantity exuded, (Figs. 9, 10, 11.) Sometimes only a few are scattered over the external surface of the vessel, (Fig. 10,) at others it exists in dense masses, filling up all the interstices between the vessels, (Fig. 11.) If examined when recently exuded, nothing but a finely granular and dense mass can be observed. At a little later period, however, we may see in the exudation round bodies, perfectly transparent, with defined edges, varying in size from the $\frac{1}{200}$th to $\frac{1}{100}$th of a millimetre in diameter, (Fig. 9.) Still later we see similar colourless bodies surrounded by a cell-wall, presenting all the characters of a nucleated corpuscle, containing several minute granules, (Fig. 10, *a.*) At the same time other cells are apparent, whose walls are completely distended with those granules, and the transparent body (nucleus) is obscured, (Fig. 11, *a.*) We now find that the exudation is broken up, the granules are separated from each other, although masses more or less large adhere together, and may be distinguished from the nucleated cells, by their irregular form and edge, as well as by the absence of a cell-wall, (Fig. 12, *a.*)

In this state we find the exudation presenting three kinds of objects, which I have denominated exudation granules, masses, and cells. The latter, called exudation cells, to distinguish them from the plastic corpuscle on the one hand, and pus corpuscles on the other. They are round or oval in form, their long diameter for the most part varying in size from the $\frac{1}{75}$th to the $\frac{1}{50}$th of a millimetre in diameter, although I have frequently seen them much larger, (Fig. 12.) They are frequently of a brownish or blackish tinge, and when full of granules, are so dense as to obstruct the transmission of light. The addition of water causes no change in these bodies. Acetic acid most frequently produces no change, but at others, the cell-wall is rendered somewhat transparent by it. They are soluble in solutions of potash and ammonia, in the former more rapidly than in the latter. Sulphuric ether dissolves them immediately.

On gradually pressing the perfect corpuscle, by means of a compressor, large drops, like those of oil, may sometimes be made to appear within the cell-wall, or to exude through it, and this subsequently becomes more or less collapsed. By means of friction the cell-wall may be ruptured and its contents dispersed. The nucleus under such circumstances will often be found to be very persistent.

All these appearances are best observed in the brain, on account of the facility with which the morbid products may be distinguished from the healthy structure. The same changes, however, may be seen in the lungs, although, from the very compound structure of that organ, they are not so easily determined. In the red hepatization numerous exudation granules, masses, and cells may be seen infiltrated into the pulmonary tissue, and floating loose, mixed with epithelial cells from the bronchi. In the gray hepatization, other corpuscles may be seen. These resemble in size and structure plastic corpuscles, only the cell-wall is rendered transparent by acetic acid, whilst the granules are unchanged, (Fig. 4.)

The formation of exudation granules, masses, and corpuscles, according to my observations, take place in the following manner. The exudation first coagulates in minute granules—the colourless and larger bodies soon after seen among these are nuclei, from which a cell-wall arises. During or subsequent to its growth, granules are formed between the nucleus and cell-wall.

These become more and more numerous until at length the nucleus is obscured, and the whole cell appears full of and distended with granules. In this state it assumes a brownish or black appearance; in the latter case it is opaque. The idea of Gluge, that these bodies are formed within the vessels, is evidently incorrect. Some have denied the existence of a cell-wall, but they must have mistaken the broken or disintegrated masses for the corpuscles. Henle has described them as two distinct formations. The first he calls the inflammation globule after Gluge,* the second he denominates after Vogel, granular cells.† The former he considers the primary formation, whilst Vogel considers them secondary, and the result of the latter breaking up. My own observations have led me to the conclusion, that the exudation masses which possess no cell-wall are nothing but portions of the coagulated exudation which split up and peel off from the vessels. In point of fact I have seen this take place under the microscope. The cells, on the other hand, are formed according to the general law of cytogenesis, as previously described. When these are distended with granules, the structure appears to have reached its farthest stage of development, the cell-wall now bursts or is dissolved, and its contents escape. The growth and subsequent disintegration of these corpuscles, proceeding simultaneously in the exuded mass, cause the whole to be broken up, and to become soft, pultaceous and even diffluent. In this manner inflammatory softening is produced, which may easily be distinguished from the softening of maceration or putridity by the presence of the exudation cells, masses or granules.

Exudation corpuscles and masses, such as they have now been described, are seen in greater or less numbers in the milk first secreted after parturition. They were first described by Mandl as granular bodies of the colostrum. From numerous observations made on the milk of nurses in 1841, assisted by Dr Potts, now of Jersey, I am satisfied that these granular bodies are exudation corpuscles, arising from congestion, followed by exudation into the mammary gland, (Fig. 13.)

3. *On the transformation of the exudation into corpuscles constituting suppuration, as observed in cellular tissue, or on mucous and granulating surfaces.*—The formation and collection of the fluid long known as purulent matter, is most commonly found on the surface of mucous membranes and granulating sores, and in the cellular tissue beneath the skin, or entering into the different dense organs of the body. The purulent matter formed in a phlegmonous abscess, or that discharged from a healthy granulating sore, must be considered as the type

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* Bericht, p. 182.

† Bericht, p. 19.

of this formation. Here we find it to be an opaque, unctuous, straw-coloured or slightly greenish fluid. If allowed to remain at rest, it separates in a few hours into two parts,—a thick yellow deposit, and a clear yellowish supernatant liquid, (*liquor puris.*) In this state it may be kept for months in a closed bottle without undergoing any marked change.

When collected together in any organ or tissue, so as to constitute an abscess, we find it preceded by well-marked pain, heat, redness and swelling. At first we find the part indurated, which is owing to the presence of exudation from the vessels. After a time this becomes softer and softer, until at length fluctuation is felt, and suppuration said to be established.

Mr John Hunter, speaking of spontaneous suppuration, says, "If it is in the cellular membrane that this disposition takes place, or in the investing membranes of circumscribed cavities, their vessels now begin to alter their disposition and mode of action, and continue changing till they gradually form themselves to that state which fits them to form purulent matter; so that the effect or discharge is gradually changing from coagulating lymph to purulent matter; hence we commonly find in abscesses, both coagulating lymph and purulent matter, and the earlier they are opened the greater is the proportion of the former. This gave rise to the common idea and expression, " that the matter is not concocted;" or that " the abscess is not yet ripe." The real meaning of which is, " the abscess is not yet arrived at the true suppurative state."* The facts so accurately detailed by Hunter we are now enabled to explain more correctly. The transformation of the exuded lymph into purulent matter does not depend upon any change in the vessels, but upon a process of growth taking place in the lymph or exudation itself, by means of which numerous corpuscles are produced. The expression " the abscess is not yet ripe" is singularly correct when applied at a period when the pus corpuscles have not reached maturity.

The corpuscles in laudable purulent matter are perfectly globular in form, and vary in size from the $\frac{1}{100}$th to $\frac{1}{80}$th of a millimetre in diameter. Their surface is finely granulated. They have a regular well-defined edge, float in the *liquor puris*, and roll freely upon each other. On the addition of water, they become much increased in size, their finely granular surface disappears, and they become more transparent. Weak acetic acid partially, and the strong acid completely, dissolves the cell wall, and brings into view the nucleus, which generally assumes the appearance of two or three granules close together, each with a central shadowed spot They are generally about the $\frac{1}{400}$th of a millimetre in diameter.

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* On the suppurative inflammation.

Occasionally four or even five granules may be observed. Alkalies and ether entirely dissolve them.* (Fig. 15, *a.*)

On examining the masses of lymph occasionally evacuated with purulent matter from an abscess, not yet ripe, they will be found to consist of a finely granular matter; occasionally larger granules may be observed in them sometimes arranged in twos and threes, similar to the nuclei observed in the pus corpuscle. Vogel has figured these nuclei in the exuded mass surrounded by a transparent cell wall.

With respect to the manner in which pus corpuscles are formed, the observations of Vogel, Henle, Leehmann, and Messerschmidt, agree in supporting the view that they arise in the coagulated exudation as a blastema. The central granules constitute a nucleus from which the cell wall arises until the perfect corpuscle is produced. This process going forward in a vast number of such organisms, break· up the solid fibrinous mass, which becomes fluid, and at length converted into purulent matter. In the fluid produced by blisters, and in the early stage of pustules formed by frictions with tartar emetic ointment, molecules and delicate corpuscles, varying in size and in all stages of development, may be observed. On the addition of acetic acid their nuclei become beautifully apparent. Pus corpuscles generally evacuate themselves externally, but occasionally, as we shall afterwards see, they are broken down and absorbed.

According to Vogel, purulent matter may be formed both from exudation rendered solid and from the fluid blood-plasma before coagulation. It is formed in the latter way principally on mucous or granulating surfaces, as well as in fistulæ and on the lining membrane of abscesses, when the original collection of matter has been evacuated; in both cases, however, the process of formation is the same, only the blastema being more consistent in one than in the other. The idea put forth by Vogel, that the pus corpuscle is a species of epithelium cell may be considered correct, if by the latter term he understands any kind of cellular structure thrown off from the surface of membranes; but if it be meant that the normal epithelium cell ever becomes converted into a pus corpuscle, the opinion is opposed to observation, and inconsistent with the large granulations of the latter produced. Neither have I ever seen any grounds for supposing with Gerber that they arise from the splitting up of the exudation corpuscle. It is more consistent with known facts to suppose that each of these organisms are independent structures, arising primarily in the exudation, and are not convertible into each other.

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* Gruby, Hunefield, Lehmann, and Messerschmidt, have described the effects of numerous reagents on these bodies, which will be found very useful in any inquiry into their chemical composition. See Henle's Bericht, pp. 187–197.

The different isolated corpuscles which have hitherto been described are not necessarily connected with any particular locality or structure. It is undoubtedly true that plastic cells are most commonly found on serous membranes, pus cells on mucous and granulating sores, and exudation cells in dense parenchymatous organs, yet this rule is by no means exclusive. Pus corpuscles are sometimes found in the brain, liver, or on the surface of serous membranes; plastic cells in grey hepatization of the lungs or on the surface of mucous membranes; and exudation cells on the lining membrane of abscesses, or on the surface of mucous membranes and granulating sores. This may probably be dependent on the changes produced by the inflammation itself, which converts one tissue into a structure resembling another. Serous membranes, for instance, owing to the exudation often resemble mucous, and *vice versa*. Such changes in the situation of the corpuscles, however, are comparatively rare, their seat of predilection being undoubtedly that to which we have ascribed them.

Gruby considers that the varieties in the different products of inflammation described are not so much to be sought in the corpuscles as in the fluid with which they are mixed. Thus, according to him, the corpuscles in mucous or purulent matter do not differ, but the substance in which they are found varies considerably in its consistence and other properties. Now, if we examine a portion of tenacious mucus, such as a person frequently expectorates in the morning, and which is of a greyish dirty colour and tough consistence, it will be found to contain round and oval cells, resembling in size, general appearance, and chemical properties, exudation cells. When, however, as in cases of catarrh, the expectoration becomes abundant and fluid, the corpuscles resemble those of purulent matter. Hence, then, it is true the blastema in one case is more fluid than in the other, but what we are assured of is that the corpuscles are different also. We shall refer to the probable cause of this in a subsequent part of the essay.

Before quitting this subject it is necessary to observe that the three varieties of corpuscles we have described frequently undergo important modifications. What has been said refers to what may be called the type of such formations. In individuals who labour under particular cachexies, as scrofula, syphilis, scurvy, &c., the corpuscles vary more or less in their characteristic properties. We believe, however, that the separation of the three kinds of corpuscles, which distinguish the three great products of inflammation, will facilitate the future study of the differences which these structures occasionally present. This the more so, as, according to our observations, it is rare that the addition of reagents will not enable us to refer them to one or the other of these divisions.

4. *Of the transformation of the exudation into permanent tissues.* It is a well known fact that after destruction or loss of certain parts they are regenerated, or a substance is formed which acts as a substitute for the tissue removed. For instance, a bone being broken, callus is thrown out and new bone produced. The new tissue thus formed may either resemble the structures in which they occur, or be dissimilar to them, as when a portion of skin is lost a cicatrix is occasioned. As a general rule, very complicated tissues, as the skin, lungs, or brain, are seldom regenerated, whereas, in more simple tissues, as the filamentous, fibrous, or even osseous, this is common. Of the kinds of permanent tissue reproduced the fibrous is the most frequent. It constitutes the cicatrices which replace lost tissues in the skin, lungs, liver, spleen, &c.

The process by which this regeneration of parts takes place is essentially inflammatory; that is to say, an exudation of blood-plasma from the capillaries occurs; this serves as a blastema for the formation of nucleated cells, which become transformed into different structures according to the law of cytogenesis. In the words of Vogel, the process is "the same as occurs in the organization of all the tissues, as observed in the embryo. It is the same whether this organization takes place from fluid or already coagulated exudation. So far as our observation extends, it always follows from a formation of cells. In the exudation arise *nuclei* with *nucleoli*, and these form a cell wall. Thus primary cells are produced, which, according to the general laws of organization, pass by further developement into the different tissues, as blood corpuscles, cellular tissue, cartilage, bone, nerves," &c.*

This fact admits of easy demonstration. We have already alluded to the manner in which lymph becomes fibrous tissue. It is by the same process cicatrices are formed throughout the body. If a recently formed granulation on the surface of a healing sore be examined, numerous cells will be observed, of various shapes, and in different stages of developement. Some are round, others caudate, spindle-shaped, elongated, or splitting into fibres, exactly similar to the method in which filamentous tissue is produced, as originally described by Schwann, (Fig. 8.) In all cases of hypertrophy a like process takes place; but here the changes are more gradual, and the organization of the exudation is so perfect as to be indistinguishable from the old tissue. In chronic inflammation of the stomach, or in dysentery, we find the muscular coat thickened or hypertrophied, and, on examination, we can demonstrate nucleated bands of non-voluntary muscle increased in thickness, and of great beauty. Hypertrophy of the heart, liver, or other organs is produced by the formation of new and perfectly similar structures to that already in existence. The result of hypertrophy from inflammation is well observed in bones, when, from a blow or local injury, the part becomes thickened, and, on examination afterwards, the new portion is found to be identical in structure with the old.

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* Wagner's Worterbuch, Art. Entzündung.

Much discussion has arisen on the mode in which new bone is formed. Some maintain it can only be produced by bone, whilst others assert it can also be formed by periosteum. Numerous preparations have been grouped together by the supporters of both theories, which prove that both must be right. When a fracture or injury to a bone takes place, it follows that the capillaries of the part injured are inflamed, and an exudation more or less copious is poured out. In this exudation nuclei and cells are formed, which at first resemble those found in cartilage. Bone is subsequently deposited by exactly the same process by which this tissue is developed in the embryo, the vascular membrane, whether external or internal, or lining the Haversian canals in the substance of the bone, as it contains the greater number of capillary vessels, will of course be most active in pouring out the exudation, and thus subserve the purpose of producing new bone. In this way periosteum, bone, or both may form new bone, according as the vascular structures in one or the other are most affected.

When an incision has been made in the skin, blood flows from the divided capillaries, until they are obstructed partly by their own contractility, and partly by the coagulated blood blocking up the cut extremities. The early phenomena of inflammation now manifest themselves, the capillaries become distended and engorged with blood, and at length exudation or the essential phenomenon takes place. After a time, which varies according as the blood more or less abounds in fibrin, the cut surface is glazed as it is called, that is, the exudation has coagulated on the surface, and is transformed into plastic lymph. If now the parts are accurately brought together, little more exudation takes place. Cells are formed, which rapidly pass into a fibrous formation, and healing or *union by the first intention* is the result. When, however, the lips of the wound remain apart, or there has been loss of substance, the exudation is more copious externally than internally. The portion which is infiltrated into the tissue surrounding the sore, constituting the inflamed, red, and indurated margin, is transformed into exudation cells, which, on breaking up, are absorbed. Some of these occasionally find their way to the surface, and become mixed with pus corpuscles,—a circumstance which probably, with some, has supported the supposition, that these structures are different stages of one growth. On the other hand, the exudation which is poured out externally on the surface of the sore is very abundant, and is transformed partly into the pus cells, and partly into primary cells, which are, by the process of development, converted into fibres, and ultimately constitute the cicatrix. The portions of exudation which are undergoing this process are called granulations. As these become more numerous, the amount of pus diminishes, and a greater tendency is manifested in the exudation to pass into permanent tissue. At length pus ceases to be produced,—the whole exudation passes into fibres,—a new surface is formed,—the which contracting, after a time, constitutes a cicatrix.

In this case a greater amount of fluid exudation is poured out on the surface of the sore, than into the neighbouring tissues, because externally the capillaries are more attenuated, and nothing obstructs the exudation. As new tissue is formed, new vessels are also produced, which in their turn assist in pouring out blood plasma on the surface. Internally the vessels are more uniformly supported by dense tissue, and the amount exuded in that direction is not so great. A section of a granulating sore at this time presents the following appearances.—Among the tissues constituting the edges of the ulcer, there is considerable granular exudation, with exudation cells and masses. The base is entirely made up uniformly of filamentous tissue; above of cells varying in shape and passing into fibres, and most superficially of purulent matter,

New vessels. It is now well understood that the appearance of vessels in colourless tissues, as in the conjunctiva, is not owing to their being newly formed, but to the over-distension of those which were previously too transparent to be visible. In the exudation poured out on serous membranes or on granulating surfaces, vessels which had no previous existence are produced, and the manner in which this takes place is not yet definitely determined. Two views have been advanced; 1*st*, That new vessels are formed in connection with the old ones, by the escape of a blood corpuscle, hollowing out for itself a channel in the exudation, which subsequently becomes a vessel. 2*d*, That the new vessel arises independently of the old one, from minute points which become enlarged and afterwards connect themselves with the old ones. This opinion, which was advanced by John Hunter, is most consistent with the researches of Schwann, and the known mode of development of vessels in the embryo. Doellinger and Kaltenbrenner* consider that new vessels may be formed in both stages. Their mode of arrangement in lymph have been shown by the injections of Pockels,† and in granulations by those of Liston.‡ The difficulty of the inquiry consists in ascertaining how the vascular walls are formed previous to that period when an injection can be thrown into them. The late observations of Mr Travers,[*] on the injured web of the frog's foot, seem to confirm the first view above-mentioned, whereas those of Hasse, Henle, Skoda, and Kolletscha, on the new vessels of lymph in man, tend to the conclusion that isolated extravasations of blood, channel for themselves passages in the coagulated exudation, which subsequently become vessels, and unite with those previously existing.[†] These isolated and star-shaped collections of blood I have frequently seen in recently effused lymph, but hitherto have never been able to satisfy myself that they constitute the first formation of new vessels. The whole subject demands renewed investigation.

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* On the formation of new blood-vessels in lymph, by Dr Allen Thomson, p. 21.

† Idem. p. 28.

‡ Med.-Chir. Trans. vol. v. New Series.

We have now described the modes in which the exudation becomes organized, and the modifications produced in it to constitute lymph, softening, purulent matter, granulations, cicatrization, &c. To render the account complete it would be necessary to speak of the transformation of the exudation into tumours, and that peculiar modification of it which is known as tubercle. We shall defer the consideration of these subjects, however, until another opportunity. From what has been said it will be seen that the different results which the termination of inflammation in organization produces, may be classified under the heads of temporary and permanent. The temporary structures originating in the exudation, such as plastic, exudation and purulent matter cells, when once fully formed, may be again absorbed. This termination is generally known by the name of Resolution, and it is the process by which this is accomplished, that we have next to consider.

3. *The Termination of Inflammation in Resolution.*

By resolution has always been understood the disappearance of the inflammation without causing any external lesion. It is best seen in the case of a phlegmonous abscess or bubo, which, being more or less advanced, presenting even a feeling of fluctuation, gradually becomes harder and harder, smaller and smaller, and at length disappears without breaking. In this case the abscess is resolved, in point of fact, the exudation has been absorbed.

Resolution or absorption of the exudation may occur in various ways, and follow any of the transformations of the exudation except the one which converts it into permanent tissue. The early phenomena first disappear; the capillaries recover their contractility; the attraction between the blood and parenchyma ceases; and the blood within the vessels begins to oscillate, and at length flows in a continuous stream. *Secondly*, the essential phenomenon disappears, no further exudation takes place, and that already poured out is absorbed.

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[*] On Inflammation, p. 166.

[†] Henle's Bericht, p. 206.

It occasionally, though rarely happens, that the exudation does not coagulate for some time after it is exuded. Under these circumstances, when the early phenomena terminate, it re-enters the vessels by endosmosis, unchanged. In the majority of cases, however, it coagulates, and once rendered solid, it could never be absorbed, without the occurrence of changes in it, by which it is again rendered fluid. This is effected by the formation, ripening, and disintegration or decay (moulting process, Schulz,) of nucleated cells, whereby the coagulated exudation is broken up, made soft, pultaceous, and diffluent, and at length absorbed. It is by this process that exudation poured out into the lung or brain gradually disappears, by the production of inflammatory softening, such as we have previously described it. On the serous surfaces, the fluid and broken down corpuscles are absorbed; but that portion which passes into permanent organization is transformed into fibrous tissue, becomes covered with a smooth membrane, so that the functions of the organ are not disturbed. Abscesses when resolved undergo a similar process.* The pus cells, instead of being evacuated, are brought closer together, from the absorption of the more fluid portion (*liquor puris*). These are gradually broken down, the cell walls are dissolved, and the whole is reduced to a molecular matter, which re-enters the vessels, and thus complete resolution is produced.

The disintegration of pus corpuscles previous to absorption is evidently favoured by the pressure which the abscess receives from the contraction of the filamentous and elastic tissues that form its walls. This is shown by the increased hardness which always accompanies the disappearance of suppuration by resolution, and the good effects which result from direct pressure employed by the surgeon to discuss these swellings. In Berlin, the most successful treatment for bubo consists in simply placing a stone of one or two pounds weight over the tumour in the groin, as the individual is laying on the back. It is probable, also, by increasing the contraction of the integuments, as well as by removing fluid from the neighbourhood of the part, that irritants, blisters, and cauteries are so beneficial in the resolution of abscesses.

It is suggested by Zimmerman that the formation of an acid, as the lactic, in abscesses when fully formed, favours their disintegration. We have seen that acetic acid dissolves the cell wall, and causes the nucleus to appear in the form of granules If lactic acid be produced it would probably have the same effect. Alkaline solutions also, it is well known, dissolve pus corpuscles,—a circumstance that may explain the discutient effects of alkaline lotions and washes, and their beneficial operation in removing the incrustations from eruptive pustular diseases.

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* "I have seen cases in which pints of matter have been taken up in psoas abscess, and the parietes remain without breach."—*Travers on Inflammation*, p. 186.

The next point to determine is, what becomes of the molecular fibrine, which thus re-enters the circulation? On this subject the observations of several German physicians, more especially of Schönlein and Zimmerman have thrown much light. They have observed with great care the changes which the urine undergoes in acute inflammatory diseases, and determined that these changes bear a relation to the absorption of exuded blood plasma in internal organs. Thus in a case of pneumonia, which I watched in La Charité Hospital, Berlin, under Schönlein in 1841, he pointed out that the disappearance of dulness was accompanied by a turbid state of the urine, which contained a large amount of molecular fibrine, and was also highly coagulable by heat. Similar results have been frequently observed in the clinical wards of Schönlein during the resolution of pleuritis, erysipelas, small-pox, abscesses, and other diseases. M. Martin-Solon, in his work de l'Albuminurie, 1838, observes, that in a great number of instances of acute febrile and inflammatory diseases, such as ague, typhus, measles, small-pox, febrile urticaria, pneumonia, gout, rheumatism, and inflammation of serous membranes, he has remarked that the urine for some days about the period of crisis yields a more or less abundant precipitate, which is owing to a superabundance of lithate of ammonia.* Zimmerman more especially has recorded instances where the turbidity and coagulability of the urine bore a marked relation to the diminution of suppurative swellings. In some cases where purulent matter was apparently absorbed, he had observed that the urine was coagulable from the presence of fibrin dissolved in it.† In a case of pleuritis, and another of pneumonia, where this change in the urine was observed, the blood also was found to contain an excess of fibrin.

These observations have led to the conclusion, that the molecules of the broken up exudation, after circulating in the blood, are frequently eliminated by the kidneys, and make their exit from the system by the urine, sometimes entire, at others in a state of solution. This conclusion is confirmed by a number of pathological and chemical facts, and may be considered established. Occasionally the excess of fibrin may be eliminated by the skin, lungs, and bowels. In all cases it constitutes an important symptom of the crisis.

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* See Christison "On the Granular Degeneration of the Kidnies," p. 42.

† Casper's Wochenschrift, 1843.

The morbid disposition in urine is not always the same. It is very probable that the fibrin, whilst circulating in the blood, undergoes certain changes of a chemical nature. According to Liebig, the oxygen of the blood converts the fibrin into urate of ammonia, choleic acid, sulphur, phosphorus, and phosphate of lime. Each of these undergo further changes. The urate of ammonia, by the farther action of oxygen, is converted into urea and carbonic acid; the choleic acid into carbonic acid and carbonate of ammonia; the sulphur and phosphorus into sulphuric and phosphoric acids, which, combining with an alkali or earth, form sulphates and phosphates. If it should happen that the quantity of oxygen taken in is not sufficient completely to accomplish this cycle of changes, then, instead of urea, either urate of ammonia appears in the urine, or, if the ammonia have entered into any other combinations, pure crystals of uric acid or fibrin.* In this manner we can comprehend how, after the resolution of acute inflammatory diseases, different sediments appear in the urine from the conversion of the excess of fibrin contained in the blood.

Mulder has lately shown, that the buffy coat in inflammatory blood is an oxide of fibrin. Now, it may be asked, does this excess of oxygen attach itself to the fibrin already in the blood and precede inflammation?—or does it combine with the fibrin reabsorbed after exudation has taken place? I am inclined to suppose the latter, for it is difficult to explain how the blood should receive a relative increase of fibrin before inflammation, or, as we have endeavoured to express it, its essential phenomenon, exudation, has taken place. When, however, we know that the fibrin which has been coagulated in the parenchyma of the lung in pneumonia is, after a time, re-absorbed, the presence of an increased amount of that principle in the blood is easily accounted for. We are far from wishing to attribute the whole of the increased fibrin to this source. A similar change has been observed in the mass of the blood under circumstances where the local symptoms were comparatively trifling, and where the lesion was incapable of furnishing the amount of fibrin formed, as in amygdalitis for instance. In such cases, however, according to Andral and Gavarret, the increased quantity of fibrin was small. At all events, it appears by no means improbable that the amount of fibrin thus absorbed, added to the amount previously existing, may so sur-charge the vital fluid that its excretion by the emunctories becomes visible.

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* Simon's Beitrage, p. 536.

Of secondary purulent formations.—In a former part of this essay, we stated that no one who had examined the blood corpuscles on the one hand, and the capillaries on the other, could conceive the possibility of the former passing through the latter if both structures were in a state of integrity, and that all observation was opposed to such a supposition. In like manner it is impossible that blood or pus corpuscles could pass backwards into the vessels. How, then, it may be asked, do the metastatic abscesses, or secondary depositions of pus, as they are frequently termed, which follow severe wounds and injuries, take place? Similar observations to those we have just alluded to, indicate two modes in which this occurs. Either the broken down pus corpuscles when absorbed are again deposited in other organs, and, conjoined with fresh exudation, constitute the blastema for a new formation of pus, or they pass unchanged through lacerations in vessels large enough to receive them, become obstructed in the minute capillaries, and then produce inflammation and suppuration.

The same observations to which we have previously alluded, respecting the absorption of the exudation in a molecular form in pneumonia, pleuritis, abscesses, &c., also indicate the possibility of the molecules being occasionally deposited in other organs when anything occurs which obstructs their excretion through the kidneys. It is certainly difficult to prevent hypothesis from entering into our consideration of this subject, because, hitherto, observations of the state of the blood and of the urine have been very few in such cases. We may readily understand, however, that if the excess of fibrin does not pass off in the urine or some other discharge, it may be deposited in a molecular form in other tissues, and there constitute a blastema, in which (perhaps conjoined with fresh exudation) collections of pus corpuscles arise. In this manner a secondary inflammation followed by suppuration may be occasioned.

Gruby states that the corpuscles found in a metastatic abscess are destitute of cell walls. Afterwards, according to him, an inflammation arises around the metastatic infiltration; *new* globules of pus with an envelope, molecules, and a central vesicle, shortly arise; so that even here and there, in a metastatic abscess, globules of pus with an envelope are to be detected. This, in our opinion, is tantamount to saying, that pus corpuscles are formed in the granules of the exudation deposited.

Secondary purulent formations, however, are most common results of severe injuries, such as compound fracture or laceration of parts. In such cases a large number of vessels are torn across, and we can readily understand, that if the exudation or lymph which ought to obstruct these vessels, instead of passing into permanent tissue, become transformed into pus, that the corpuscles will have a direct and ready entrance into the circulation. We shall afterwards see that a rapid and large amount of exudation, as occurs in such cases, favours this result. Zimmerman supposes that the pus already formed dissolves the coagula, which obstruct the vessels. Perhaps it would be more correct to say, that, instead of passing into tissues, the coagulated blood plasma forms a blastema for the formation of numerous corpuscles. That, under certain circumstances, this occurs even within the vessels, has been shown by Gulliver,* and I have satisfied myself of the correctness of his statements by direct observation.

In a man who died after amputation of the thigh, under Mr Syme in the Royal Infirmary, I carefully dissected out the femoral vein and its principal branches leading from the stump. In slitting up the main trunk, I found it entirely filled with a coagulum, which, in the portion of the vessel passing under Poupart's ligament, was very firm; but, on tracing it downwards, its consistence was found to diminish. About the middle of the thigh it was broken down and soft, and, towards its termination, became diffluent. That this was not a mechanical softening was proved by a careful microscopic examination. Superiorly the clot only contained the minute primitive filaments found in colourless coagula. Further down these became mixed with plastic and exudation corpuscles. The latter were very numerous at a portion of the vessel which was somewhat constricted. As the examination proceeded lower, the corpuscles became more numerous, and the filaments disappeared, until at length nothing but corpuscles, innumerable filaments, and the fluid in which they swam, were present. In another case, of heart disease, where two small tumours the size of filberts were found attached to the mitral valves, I found internally numerous exudation and pus cells. The character of the latter were determined by the peculiar nuclei which were made apparent on the addition of acetic acid. In both cases it is evident that corpuscles had been formed in the coagulum.

Now, in cases of severe injuries, it is very probable that a similar process goes on, not only in the exudation thrown out, but in that portion of it which, under other circumstances, would have closed up the ruptured vessels. Pus, then, directly enters the vessels, and is carried into the torrent of the circulation. What now happens may be well described in the words of Zimmerman. "If the entire pus corpuscles have been taken up by the venous system, they either reach the heart and pulmonary artery into the capillary structure of the lungs, or, in case the purulent deposition was seated in organs whose veins formed the *vena portæ*, they reach through these into the capillaries of the liver. Now, as the pus corpuscles are in general too large to pass through the fine capillary net-work in the lungs and liver, it is evident that, for this reason, they must begin to stop here, to accumulate, and to occasion a stasis as foreign irritants. These, then, form new pus depositions, which progressively become greater from the constituents of the blood, and of the parts of the tissue capable of it. From this simple cause, it is easily explained why secondary abscesses form so readily in the liver and lungs. If the pus deposited in the liver, and newly formed, be taken up by the hepatic veins, these bring it into the heart and into the lungs. If it be taken up from these by the pulmonary veins, it may be deposited in every other organ to which the blood comes from the left ventricle. In this case secondary abscesses may form in the brain, in the kidneys, in the spleen, &c. and pus may even be discovered in the urine.*"

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* Gerber's Anatomy, note, p. 31.

Purulent matter formed within the vessels, the result of arteritis or phlebitis, produces similar results in a like manner.†

VII.—THE CIRCUMSTANCES WHICH INFLUENCE THE TERMINATIONS OF INFLAMMATION.

It becomes very important to inquire what are those circumstances which induce the different transformations of the exudation, such as we have previously described them, and when we may expect lymph, softening, suppuration or permanent tissue to follow the inflammatory process. A satisfactory elucidation of this subject must be intimately connected with our mode of treatment, inasmuch as a knowledge of those circumstances which favour or retard one more than another, will enable us in particular cases to apply our remedies so as best to assist the intentions of nature.

It at once becomes evident that the essential cause of organization lies in the vitality and peculiar properties of the exudation itself. This, like every blastema, as the egg in animals or the seed in plants, possesses the power of developement, and passes into organization when placed in circumstances favourable to growth. It is not connected with the causes of the inflammation, as these, however various, whether chemical, mechanical or vital, give rise to similar results. The origin of this function, therefore, must be sought in the vital properties of the exuded blood plasma alone. The same general circumstances also influence the development of the exudation as influence organization in every species of blastema, whether vegetable or animal. The most important of these are temperature, the presence of moisture, and certain changes of the atmosphere.

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* See translation of Zimmerman's paper, Med. Chir. Review, July 1844, p. 152.

† See translation of Gruby, Obs. Micros. Journal, Vol. ii. p. 266.

That temperature exercises a most powerful influence on the growth of plants, that is, on the development of the cells of which they are composed, is a well-known fact. All vegetation requires a temperature between 0° and 100° Fahrenheit. It is also well established that vegetation is more rapid in elevated than in low temperatures, and horticulture teaches us that its proper regulation will enable us to accumulate within a small compass the plants of every clime. The importance of temperature in the growth of animals is equally well marked, and may be observed in the process of incubation, and the care with which the mother instinctively guards her young from the approach of cold. Even in man, the fact that he attains maturity earlier in warm than in cold climates, is only a further illustration of the same general law. Growth, as we have seen, only consists in the evolution and further development of nucleated cells, and the same laws which apply to this process in plants and animals in a state of health, apply to them in disease.

It has long been recognized in practical medicine, that cold and heat locally applied, are amongst the most powerful means of combating inflammation. It is well known that the first checks the inflammatory action, and retards the formation of pus, whilst the last favours the passage of the exudation into suppuration. This is only acknowledging that cold checks, whilst heat facilitates the passage of the exudation into nucleated cells. In short, for the same reason as the horticulturist who wishes to bring forward a plant, places it in a hot house, so the surgeon who desires to bring on suppuration applies hot poultices and fomentations. From these considerations the indications for the especial application of cold and heat in inflammation may be at once deduced.

That moisture is as necessary for growth as a certain temperature may be proved from like observations in plants and animals. I need not allude to the necessity of supplying water in order to secure the growth of the former, or to the circumstance that in every ovum a fluid surrounds the yolk, or portion where nucleated cells are actively developed. In the same manner we have seen that a rapid exudation and consequent excess of fluid favours the evolution of isolated cells, as those of pus or of plastic lymph, whilst its diminution is more favourable for more permanent growths. The formation of pus seems to be more especially favoured by the constant addition of fluid, and hence probably its common occurrence in all those situations where this takes place, as from the walls of abscesses, the surface of mucous membranes, ulcers, &c. For this reason suppuration is impeded, and union by the first intention favoured, if, before uniting the edges of incisions, we wait till the extravasation of blood has ceased, and exudation has occurred. Hence may be explained the practice so long prevalent in the Edinburgh School of Surgery of not sewing together the flaps till some hours after amputation, and glazing has taken place. On the other hand, moist applications conjoined with heat have been found most favourable for advancing suppuration.

It was supposed by Hunter that the presence or absence of atmospheric air exercised an influence on the results of inflammation. To the seclusion from air he ascribed the production of lymph, as in shut cavities, whilst that of pus, according to him, is favoured by an exposure to it. A free surface, as exists on the skin or mucous membranes, as they favour an effusion of serum or excess of moisture, so they naturally favour the passage of the exudation into pus or isolated cells. Hence, probably, the entrance of air into large abscesses, so much dreaded by surgeons, is not injurious from any poisonous influence it exerts, as from the circumstance that it keeps the walls separate, and thus favours an excess of exudation and organization.

That changes in the atmosphere, independent of temperature and moisture, exert a powerful influence in advancing, impeding, or destroying growth, becomes evident on paying attention to its effects throughout all animate nature, vegetable as well as animal. Crops which have called forth the utmost skill of the agriculturist, and which seem to promise the best return, slowly or suddenly become withered; their further growth is checked, and they die, and this not owing to any fault in the soil or in the nature of the seed, but to some yet undiscovered change existing in the atmosphere. So in particular seasons of the year, or in certain localities, we observe epidemic or hospital gangrene, and witness the same exudation which, under other circumstances, would become organized, together with the structures involved in it, &c. become mortified and thrown off from the economy. Of the nature of this atmospheric influence or malaria we know nothing, but that it exists, observations such as we have alluded to sufficiently prove.

Why the exudation should not always undergo the same transformations or pass into the same organization, it becomes very difficult to determine. The blood plasma exuded throughout the body, when first poured out, is, in all tissues, essentially the same, and yet we see plastic, exudation, and pus cells formed in it, and frequently cellular tissue, bone, or other structures. Occasionally we see the same exudation partly transformed into pus, and partly into permanent tissue, as on the surface of an ulcer. All that can be said on this subject appears to be, that the same vital laws, which, in a healthy state of the economy, produce from the same blastema nucleated cells, passing into fat, muscle, nerve, bone, and so on, induce, in a state of disease, results equally dissimilar from the same cause. Both research and observation, however, indicate that there are certain circumstances connected with the *individual* which exert a considerable influence in determining the kind of organization which takes place in the exudation. These circumstances appear to be connected, 1*st*, with the elementary structure of the tissue; 2*d*, with the vital power of the whole organism; and, 3*d*, with the progress of the inflammation. As these are points of great practical importance, I shall shortly consider each in succession.

1. The surrounding elementary structures exercise an evident influence on the further developement of the exudation. Thus, blood-plasma effused in the neighbourhood of cellular tissue, is again developed into cellular tissue, as we see in granulations, in the greater number of regenerations and healing of wounds. Exudation in the immediate neighbourhood of bones, or into their substance, is changed into bone. Even the primitive fibres of nerves have been said to be capable of regeneration when once divided. Serous membranes, shut sacs, with vessels and epithelium, may be newly formed from exudation effused on serous surfaces. Every tissue is capable of being hypertrophied, when the exudation is poured out slowly, as in chronic inflammations. In all these cases the new pathological formation depends apparently upon the influence the surrounding parts possess over the other circumstances which govern the developement of the exudation.

2. The vital power of the whole organism has a most undoubted influence over the further developement of the exudation. When inflammation is followed or accompanied by general loss of strength, it does not become organized, or is very imperfectly so. In such cases the exudation breaks down into an undetermined granular mass, although it sometimes exhibits an imperfect tendency to cell formation. Diminished energy of the vital power, therefore, or certain changes in the constitution of the blood, tends to reduce and destroy the further developement of the exudation. This is very well observed in scrofula, in which disease, whilst the fibrin of the blood is diminished, the

albumen is in excess. Here, when suppuration takes place, it occurs slowly, and the pus cells are found to be imperfectly developed. Instead of being round and rolling freely on each other, they are angular, collect into masses, and are mixed with numerous granules. In parenchymatous organs, the occurrence of tubercle is common, which consists of numerous granules, with a few imperfect attempts at cell formation. In syphilis, scurvy, and other cachectic constitutions, it is well known that the exudation passes very slowly into organization, and that it frequently dies, causing ulceration. Moreover, the difficulty with which fractures and wounds heal in individuals badly nourished, in old age, or in such as labour under putrid diseases, as typhus or jail fever, sufficiently demonstrate the influence of the general vital powers on the development of the exuded blood plasma. *

3dly, The rapidity with which the exudation takes place, and its amount, exercises a powerful influence on its organization. When the inflammation is acute, when the exudation occurs in considerable quantity, and the power of the constitution offers no check to the development, it passes readily into exudation, plastic and pus cells, and if there be loss of substance this is quickly restored. The quantity and quickness of the exudation favours its transformation into one of the three kinds of isolated corpuscles, according to the texture of the part. On the other hand, when the inflammation is chronic, and the exudation is poured out slowly or in small quantity, it possesses an evident tendency to pass into a higher state of organization, and the cells are developed into fibres, or some of the permanent tissues. Hence union by the first intention from the small quantity of the exudation—union by the second intention from the slow progress of the organization, that which takes place rapidly being thrown off in the form of pus. Hence also the slow formation of adhesious and dense membranes in shut cavities. Lastly, when the exudation is poured out with great slowness, tissues are formed which are identical with those existing in a normal state, giving rise to hypertrophy in different organs. In this manner the violence of the inflammation and amount of exudation exert a great influence over the products produced, as has been long recognized in pathology by the terms adhesive inflammation, suppurative inflammation, &c.

Such, then, appear to be the more important circumstances which influence the terminations of inflammation.

We have now endeavoured to describe the principal phenomena which characterize the inflammatory process, and the manner in which its products are produced. The early phenomena, consisting of contraction, succeeded by dilatation of the capillaries, are analogous to what are called spasms and paralysis in the muscular tissue. The enlargement of the vessel permits a greater quantity of blood to enter it, whilst its corresponding increase of tenuity permits fluids to be exuded through its walls, which, in a state of health, were retained. This increased exosmosis, the approach of the yellow corpuscles to the sides of the vessel, and the stoppage of the blood, we endeavoured to show, could only be explained by supposing the existence of a vital power causing an increased attraction between the blood and surrounding parenchyma. This hypothesis is, of course, only intended to express a certain series of facts, in the same manner that we speak of electrical attraction and repulsion, or of gravitation, as explanatory of well known occurrences.

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* May not the difficulty with which wounds heal by the first intention in Paris, be ascribed to a constitutional cause?

The early phenomena may be followed by the extravasation of blood, by the effusion of serum, or the exudation of blood-plasma. The latter constitutes the essential phenomenon, and is invariably present, serving to distinguish inflammation from mere congestion and all other morbid processes.

The blood-plasma exuded, and the textures imprisoned in it may die and pass into decomposition, rapidly, constituting mortification, or more slowly causing ulceration. On the other hand, it may assume an active power of growth, in which case different kinds of nucleated cells are formed, which either remain isolated or pass into organization of tissue. Inflammatory softening is caused by the formation of exudation cells; organizable lymph by the formation of primitive filaments, mixed with plastic corpuscles, and suppuration by the formation of purulent matter corpuscles. When there has been loss of substance, the exudation passes partly into purulent matter and partly into cellular tissue, and sometimes into other elementary textures. This, however, always by the process of cytogenesis. Occasionally the exudation is absorbed, either directly before its coagulation, or more commonly after this, by the disintegration of the isolated and temporary corpuscles which are formed, so that the coagulated blood-plasma is again rendered fluid and molecular, and capable of re-entering the vessels. These different results constitute the subsequent phenomena, or terminations of inflammation.

Lastly, we have seen that the vitality of the exudation is influenced by the existence of a medium temperature, moisture, and the state of the atmosphere, and that the kind of organization into which it may pass is dependent, 1*st*, On the elementary structure of the tissue in which it occurs; 2*d*, By the vital power of the whole organism; and 3*d*, By the progress of the inflammation.

VIII.—CONCLUDING REMARKS.

Before altogether closing our consideration of the pathology of inflammation, it will be well to allude to one or two points which we may now discuss with some hope of arriving at a satisfactory conclusion. One of the first pathologists, and one of the first physiologists in Europe, viz. Andral and Magendie, have both declared that the word inflammation should be banished from medical nomenclature, " created," says Andral, " in the infancy of science, this expression altogether metaphorical, was destined to represent a morbid state, in which the parts appeared to burn, to be inflamed, &c. Received into general language without any precise idea having ever been attached to it, in the triple relation of symptoms which announce it, of the lesions which characterize it, and of its intimate nature, the expression inflammation is become so very vague, its interpretation is so very arbitrary, that it has really lost its value, it is like an old coin without an impression, which ought to be removed from circulation, as it only causes error and confusion. Magendie remarks, " when we are called upon to pronounce the word inflammation, it is rather to criticize it than to attach to it any definite ideas." Again he says, " one could fill an entire book with all the ideas which represent the word inflammation, for it is synonymous with the word disease."

Andral, after having pointed out the confusion resulting from the term inflammation, has replaced it by the word *hyperæmia*, or excess of blood. We evidently, however, gain nothing by this substitution, as it is one of words, not of ideas, and the term congestion, already in use, is synonymous with it. Magendie, in ridiculing the word inflammation, has never attempted to replace it in any way. Eisenmann has endeavoured to substitute the word *stasis*. But this only means stoppage of the blood, and may take place in the veins as well as in the arteries, without exciting any of the phenomena called inflammatory. Hence it has been the object of British pathologists to give precision to the old term inflammation, rather than change it for another, perhaps more unsatisfactory.

It is evident, however, that the symptoms of pain, heat, redness, and swelling, are no longer sufficient to indicate inflammatory action. All these may depend upon simple congestion alone, and they may all be absent even when an inflammation has been sufficiently powerful to destroy life, as in the substance of the brain. Neither can we any longer retain the vague ideas which exist with respect to its pathology. Most pathologists, indeed, on being questioned, are obliged to acknowledge that of this they know nothing, and practical men, though they profess themselves capable of detecting its existence, are all ready to confess their ignorance of its nature.

One of the most powerful opponents of the ideas of Andral and Magendie, is the present distinguished Professor of the Practice of Physic in this University. Dr Alison says "A peculiar perversion of nutrition or of secretion, we hold to be essential to the very existence of inflammation; and all descriptions, and all attempts at explanation of the changes to which the term is applied, if they do not include this, their most essential peculiarity, we must regard as necessarily and fundamentally defective."* Now it is this perversion of nutrition that I hold we must regard as the principal feature in all the phenomena of inflammation. In order, however, to understand what is meant by perversion of nutrition, it evidently becomes necessary that we should have a clear idea of what is meant by healthy or normal nutrition.

We have taken great pains to point out that a healthy nutrition consists, 1st, in a healthy state of the blood; 2d, in a healthy exudation of blood-plasma; and 3d, in the formation of nucleated cells, passing into the healthy structures of the organism, as fat, bone, muscle, and nerve. In inflammation, however, we have seen that there is, 1st, an unhealthy state of the blood; 2d, an unhealthy exudation of blood-plasma; and 3d, a formation of nucleated cells, passing into structures foreign to the organism, as softening, lymph, purulent matter, and cicatrix. Surely, then, we are warranted in saying that inflammation is nothing more or less than a form of anormal nutrition.

Dr Alison further observes, "It is true that the various effects which we ascribe to inflammation, adhesion, suppuration, ulceration, and gangrene, are very different from one another, and that we cannot satisfactorily point out the cause or even the mode by which each is effected." It is hoped, however, that in the preceding pages, not only has a sufficient cause been adduced, but that the mode in which the different terminations of inflammation are produced has been described from actual observation. That is, in the same manner that Schleiden in plants, and Schwann in animals, have indicated the steps of normal nutrition, owing to the formative influence of a vital blastema, so it has been endeavoured to point out the mode in which anormal nutrition acts in producing the various results of inflammation, from the transformations of the exudation.

It may be thought by some that this doctrine contains nothing new,† that many authors have said the same thing; that Andral, Lobstein, and numerous others in their works on pathology, have distinctly spoken of increased and diminished nutrition; and that the term morbid growths, as applied to tumours, is a sufficient proof of this. No doubt the idea of a morbid nutrition has occurred to writers on pathology, yet how have they applied such ideas to the phenomena of inflammation? So far from their having conceived purulent matter to be connected with this process, they supposed it to be secreted from the blood, in some unknown way, in order to be removed from the system. That is to say, they classified pus with the bile, urine, milk, and sweat, fluids secreted by glands to be excreted from the body. They had no idea that purulent matter was the result of an active process of nutrition, formed and developed in exuded blood-plasma as from a blastema. Even an authority so late as Gerber describes the pus corpuscle as being formed by the breaking up of the exudation cell, and Travers speaks of lymph being the secreting organ of purulent matter.* No one has clearly distinguished the difference between the cells found in pleuritic lymph and those of purulent matter, although the former have been accurately figured by Gulliver and Vogel. Again, inflammatory softening was confounded with that lesion which is the result of maceration, and no means existed of distinguishing one from the other. The identity between exudation corpuscles and the granular bodies of the colostrum was not known.† In short, it was utterly impossible that before the doctrine of growth by cells they could have formed the most remote conception of the manner in which these morbid appearances were produced. Even the steps of healthy nutrition were unknown; how, then, could they explain the morbid phenomena? Their hypothesis, however, remained, and as so often occurs in the history of science, what at one period was the merest hypothesis, and only the offspring of a luxuriant imagination, becomes confirmed by observation, and, after a time, received as established truth, I shall be satisfied if, in the foregoing essay, the mode in which this anormal nutrition takes place has been clearly shown, and the various phenomena of inflammation satisfactorily demonstrated to be explicable according to the present state of science.

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* Lib. of Medicine, p. 53.

† My first paper on this subject was read to the Medico-Chirurgical Society of Edinburgh, November 1842, and an abstract of it was published in Cormack's Journal for December following. It is referred to by Henle in his Report, p. 198.

It may further be objected, that the term anormal nutrition, if correct, can only affect the results of inflammation, and not inflammation itself. But what is inflammation unconnected with exudation? Dr Alison directly tells us that he can form no idea of inflammation unconnected with effusion. If we take away the results, we have only congestion or the early phenomena remaining, to which we can never apply the term. But it is evidently erroneous to split one process into two; and to call the first part the cause and the other the result. Besides, where shall the division be made? Who has ever thought in healthy nutrition of making such distinctions? The whole is one process, consisting essentially in the exudation of blood-plasma, and of its subsequent transformations, and so it is maintained in anormal nutrition; the exudation and subsequent changes are one process also.

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* On Inflammation, p. 122.

† See Wagner by Willis, p. 429.

If, however, the word inflammation must be used, (and it may be questioned whether the time has yet arrived for removing it altogether from practical medicine,) it must be limited to that series of actions which terminates with the exudation of blood-plasma in unusual quantities. In this sense it may always be employed with propriety, and we may always understand by it that a congestion has terminated in exudation, which may die, or become organised, be absorbed, or pass into permanent tissue, according to those vital laws which govern normal as well as anormal nutrition.

Description of the Plate.

Fig. 1. An exact copy of a portion of the web in the foot of a young frog, after a drop of strong alcohol had been placed upon it. The part was illuminated by the reflected light of day, and considerable pains have been taken to imitate the colours, such as were then presented. The view exhibits a deep-seated artery and vein, somewhat out of focus; the intermediate or capillary plexus running over them, and pigment cells of various sizes scattered over the whole. On the left of the figure the circulation is still active and natural. About the middle it is more slow, the column of blood is oscillating, and the corpuscles crowded together. On the right congestion, followed by exudation, has taken place, constituting inflammation of the part.

a. A deep-seated artery, out of focus, the rapid current of blood, allowing nothing to be perceived but a reddish, yellow broad streak, with lighter spaces at the sides. *b.* A deep-seated vein, partially out of focus. The current of blood is of a deeper colour, and not so rapid as that in the artery. It is running in the opposite direction. The lymph space on each side, filled with slightly yellowish blood plasma, is very apparent, containing a number of colourless corpuscles, clinging to or slowly moving along the sides of the vessel. *c.* In the portion of capillary plexus included within this bracket the circulation is still active and natural. The normal amount of yellow and colourless corpuscles are seen within them. *d.* In the portion of capillary plexus included within this bracket, the yellow corpuscles are crowded together, and the column of blood is oscillating. At *e*, this oscillation is very slight. At *f*, the corpuscles are pushed forwards and backwards to the extent of half an inch. At *g*, laceration of a capillary vessel has produced an extravasation of blood, resembling a brownish-red spot. At *h*, some of the yellow corpuscles less aggregated together are observed. *i.* In the portion of capillary plexus included within this bracket, congestion has occurred, and the blood corpuscles are apparently merged into one semitransparent, reddish mass, entirely filling the vessels. The spaces of the web, between the capllaries, are rendered thicker and less transparent, partly by the action of the alcohol, partly by the exudation. This latter entirely fills up the spaces, as at *k*, or only coats the vessel as at *l*. At *m*, a portion of the external epithelium is brought into focus, exhibiting its peculiar cells with their nuclei. These nuclei will be observed to resemble the colourless corpuscles of the blood, for which they have been by some mistaken.

Fig. 2. Primitive filaments in a portion of semitransparent lymph exuded on the surface of the pleura. These delicate filaments will be observed in some places to be molecular, and to contain here and there minute granules.

Fig. 3. Primitive filaments and plastic corpuscles, from a portion of opaque lymph, recently exuded on the pleura. The filaments are here stronger and more densely crowded together.

Fig. 4. Plastic corpuscles with epithelium cells. From gray hepatization of the lungs. *a.* Appearance after the action of acetic acid.

Fig. 5. Isolated cells, of various forms and in different stages of development, passing into fibres. From minute vegetations encrusting the base of the mitral valve.

Fig. 6. Similar cells, from lymph exuded on the pleura.

Fig. 7. Similar cells. Both isolated and in mass, from a layer of lymph on the peritonæum.

Fig. 8. Similar cells, from healthy granulations following a burn.

Fig. 9. Exudation in a finely granular or molecular state, with nuclei apparent, coating a vessel. Found in cerebral softening.

Fig. 10. Exudation granules and cells, coating a vessel. From a portion of inflamed placenta.

Fig. 11. Exudation granules and cells, filling up the interstices between the vessels. From a portion of inflamed brain. The addition of acetic acid has rendered the vascular walls transparent and their nuclei apparent.

Fig. 12. Exudation corpuscles from the brain. *a.* Exudation mass.

Fig. 13, Exudation corpuscles and masses, commonly called granular bodies of the colostrum. From diseased human milk, secreted some days after parturition.

Fig. 14. Corpuscles in thick viscid mucus, expectorated early in the morning by an individual in good health. Molecular filaments and granules are also apparent.

Fig. 15. Tabular view, showing the action of water and acetic acid on the plastic, pus, exudation, and colourless blood corpuscles.

ART. VI.—*Memoir on the Sex of the Child as a cause of Difficulty and Danger in Human Parturition.* By JAMES Y. SIMPSON, M.D., F.R.S.E., Professor of Midwifery in the University of Edinburgh, &c., &c.

MY object in the following observations is to show that the Sex of the child has a very marked and demonstrable influence upon the difficulties and dangers of human parturition, in relation both to the fate of the mother and of the infant.

Two winters ago I had occasion to undertake some investigations into the causes of the difficulties attendant upon parturition in the white, as compared with the black tribes of mankind; and in the human female, as compared with the female of the lower animals. In consequence of these investigations I was led to conclude that the adaptation of the head of the fœtus to the maternal canals is so very close and accurate in the process of parturition as it occurs in the female of our own race, that deviations of a very slight degree in the relative size of the cranium of the child, and of the pelvic passages of the mother, should, when viewed on the large scale, lead to differences of a very appreciable and notable extent in the relative safety or danger of the whole process.

On further considering the subject, it appeared to me that in the slight difference which is known to exist between the size of the head of the male and female child at birth, and in the effects which this difference might or might not be traced to produce upon the results of parturition,—a criterion existed by which the truth of the opinion in question might be fully tested. It was at the same time manifest that the subject was one of such a nature that the only evidence by which it could be settled obviously consisted of an appeal to an extensive body of statistical facts. The first difficulty was the difficulty of obtaining these required data. The numerous and excellent reports of instances of morbid and instrumental labour published by various authors, are, however valuable in other respects, totally useless in regard to the present subject,—in consequence of the sex of the child being rarely or never mentioned by them in the details which they give of their individual cases. The only exception to this general remark that I am acquainted with is to be found in the admirable Practical Treatise of Dr Collins.* In his invaluable record of the morbid cases which occurred in the Dublin Lying-in Hospital during his Mastership,† Dr Collins gives, at the end of the different chapters that treat of the individual difficulties and complications attendant upon labour, a table showing, amongst other items, the *sex* of the infant in each case. Dr Collins himself properly suggests, that "an extensive record" of the kind which he has published might "materially assist the physician in obscure investigations," (Preface, p. 1.) For the data forming the grounds of the following calculations I am, as will be seen in the sequel, principally indebted to the facts recorded in Dr Collins' work, and while the inferences I am about to draw form one instance of the fulfilment of his own suggestion, regarding the utility of such data as he has collected, my deductions will, I doubt not, be regarded as the more valuable and trustworthy, seeing that the facts on which they are chiefly founded were originally collected and published by Dr Collins without apparently any view whatever to the special inquiry upon which we propose to enter.

Having made these preliminary remarks, I shall now proceed to show that the sex of the child exerts a manifest influence upon the general maternal mortality accompanying human parturition, upon the frequency and fatality of its individual morbid complications, and upon the safety and life of the infant itself, both during birth and for some time subsequently to it. In all of these respects the birth of male children is attended with much greater danger than the birth of females. In proof of this, I have to adduce different series of corroborative details. These details will probably be exhibited with the greatest clearness, in the form of such a series of propositions as the data which I have to bring forward may seem to warrant. And I may further arrange these propositions into more general heads or chapters, so as to show the effect, during parturition, of the sex of the child as bearing—1. Upon the safety and life of the mother; and—2. Upon the life and safety of the infant itself;—3. I shall enter into some investigations with the view of developing the cause of the greater comparative dangers accompanying the parturition of male children; and, *lastly,* I shall allude to some practical inferences, and attempt to point out the extent to which the mortality in childbirth and early infancy is influenced by the mere sex of the offspring.

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* "A Practical Treatise on Midwifery, containing the result of 16,654 births occurring in the Dublin Hospital, during a period of seven years, commencing November 1826." London, 1836.

† "*Master* is the title of the physician to whose care this hospital is entrusted for a period not exceeding seven years." See Dr Collins' preface.

CHAPTER I.—THE DANGERS AND DIFFICULTIES OF PARTURITION ARE GREATER TO THE MOTHER IN MALE THAN IN FEMALE BIRTHS.

The greater danger and difficulties which the mother incurs in connection with male births may be demonstrated by an appeal both to the general mortality among parturient mothers, and by a reference to the sex of the infant in a variety of the most formidable complications that are found to accompany the process of labour.

One or two propositions, and the statistical proofs on which they are founded, will illustrate this remark.

First Proposition.

Of the mothers that die under parturition and its immediate consequences, a much greater proportion have given birth to male than female children.

During Dr Collins's term of mastership 16,414 women were delivered in the Dublin Lying-in Hospital. Of these, 164 died. Of the 164 that died, 7 had been delivered of twins, and 157 of single children. In searching for the effect of the sex of the infants in relation to the maternal mortality, I shall on this, as on other occasions, in the following remarks, reject from the calculations the twin cases, because the plurality of the offspring is an element that would obviously disturb and pervert, in different ways, the accuracy of any investigations like the present; for the fœtuses, in compound births, are frequently of opposite sexes,—they are in general considerably below the average size and weight,—and the expulsion of the second of them, when once the maternal canals are dilated, is not attended with such obstacles, nor hence with such danger, as the passage of the first or the transit of a single child. If we confine, then, our attention to the 157 cases in which the mothers died in connection with the birth of a single child, we will find that in 3 the sex of the infant was not noted; in 105 it was male; and in 49 female. The number of maternal deaths was therefore much higher after male than after female births. But, to show with more clearness and precision the relative excess of mothers that died after giving birth to male, as compared with those that died after giving birth to female children, I shall, on this as on other occasions throughout the present essay, compute the casualties connected with female births as equal to 100, and calculate the proportion of those connected with male births in reference to this fixed standard. In this manner the absolute numbers and relative proportion of maternal deaths, after male and female births, contained in Dr Collins' returns, would, according to the above data, stand as follows:

Total maternal deaths.	In these the sex of child		Or in proportion of male to female as
	Male.	Female.	
154	105	49	214 to 100

It is proper further to remark, that 38 out of the 157 cases appear to have died from "causes not the result of childbirth." I have not been able, from the details given by Dr Collins, to separate these 38 cases from the general number, but if they were so subtracted, the remaining 119 cases would not improbably shew a greater proportion of male births in connection with those maternal deaths that thus directly resulted from parturition or its immediate effects.

Second Proposition.

Among labours presenting morbid complications and difficulties, the child is much oftener male than female.

This proposition may be proved by a reference to many of the more formidable accidents that are found to accompany labour, as convulsions, laceration of the uterus, post-partum hæmorrhage, instrumental cases, &c.

Convulsions. Of 28* cases of convulsions described by Dr

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* Here, as in most other cases, our calculations include only, (as already stated,) the results of uniparous births.

Collins, in 17 the offspring was male and in 11 female; of the male children, 8 were still-born; of the female, 4.

Rupture of the Uterus. This formidable accident occurred in 34 instances during Dr Collins' mastership. In 23 cases the rupture was in connection with the birth of male children, and in 11, or in less than the third of the whole, it occurred in connection with the birth of female children.*

Puerperal Fever. From November 1826 to November 1833, 88 patients were attacked with puerperal fever in the Dublin Lying-in-Hospital. Among these 88 women, 54, or five-eighths, had given birth to male, and 34, or three-eighths, to female children.

Post-Partum Hæmorrhage. This complication is known to occur especially after those confinements in which the contractile powers of the uterus have been unusually taxed and exhausted during the previous stages of the labour. Dr Collins reports 44 cases of hæmorrhage after the expulsion of the placenta. In 31 of these the child was of the male, and in 13, or less than one-third, it was of the female sex.

Tedious and Difficult Labours. The duration of the labour was noted by Dr Collins in 15,580 cases. In 264 of these cases the process was prolonged beyond 24 hours. Dr Collins has not, in his chapter on tedious labours, published the particulars of all these 264 instances, nor given such data as to enable us to ascertain the sex of the children in them. These data would, in all probability, have shown a great overproportion of male infants,—more especially in that class of instances in which the delay was produced by protraction of the second stage of labour, in consequence of want of proper relative size between the fœtal head and the maternal passages.

In another part of his work Dr Collins details a set of facts that may in a great measure fill up the deficiency to which we allude. In his chapter on "Still-born Children" he notes a variety of important circumstances, relative to 1121 cases in which the child was dead at birth. He further observes, p. 462, "In 106 of the 1121, the labour was extremely severe, and in nearly half of these the patients had been *one*, *two*, *three*, days ill, or even more, before admission into the hospital, and most of them grossly mismanaged." He gives a short outline of 49 of the 106 cases of tedious labour in the General Table on Still-born Children, and marks out by asterisks 60 additional instances, which were, (to use his own words,) "similar in most respects to those detailed," p. 485. These two sets of cases of protracted labour with death of the infant, amount in all to 109 instead of 106.* Out of this number the sex of the child was male in 65, and female in 44.

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* Dr Collins adduces 20 cases of rupture of the uterus mentioned by Dr M'Keever, and out of these, in 15 the child was male, and female in the remaining 5, or in about one-fourth of the whole.

Forceps Cases. The forceps were used in 24 cases during Dr Collins's mastership. Of the children thus delivered, 16 were males, and 8 females. (See pp. 11 and 15.)

Crotchet Cases. "Of the 16,654 births which occurred in the hospital, in 79, (says Dr Collins, p. 486,) delivery was effected by lessening the head, on account of extreme difficulty in the labour, or where the child was dead and interference necessary for the patient's safety." In the section on Still-born Children 41 of the 79 are recorded, and references given to other 33 of the cases, (see p. 487.) Among these 74 crotchet cases that are thus noted, the offspring was male in 50, and female in the remaining 24 instances.

If we attempt to throw the proofs that we have collected of our present proposition under the seven preceding heads into a tabular form, and, taking again, for the facility of comparison, the female births at the fixed standard of 100, calculate the proportion of males to them, we shall find the result to be as follows:—

Nature of Complication.	Total Cases.	No. Male Children.	No. Fem. Children.	Propor. of Males to Females as
Tedious Labours,	109	65	54	148 to 100
Convulsions,	28	17	11	153 to 100
Puerperal Fever,	88	54	34	161 to 100
Rupture of Uterus,	34	23	11	207 to 100
Post-Partum Hem.	44	31	13	240 to 100
Forceps Cases,	24	16	8	200 to 100
Crotchet Cases,	74	50	24	208 to 100
Total	401	256	155	165 to 100

It is unnecessary to qualify the proof which the above table affords of the proposition that we have laid down by venturing to offer any comments. It is proper, however, to add that here, as elsewhere throughout the calculations of the present essay, it should be held in recollection that about 6 per cent. ought always to be deducted from the column of the male children, in consequence of there having occurred, (in concurrence with an acknowledged law in human statistics,) that proportion of male over female births during Dr Collins' term of mastership in the Dublin Hospital. He noted 8548 male births, and 8068 female births, being in the proportion of 106 males to 100 females.

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* In 97 of these 109 instances of tedious labour, the number of hours during which the patients were in labour is stated. Five were under 12 hours in labour; 14 were from 12 to 24 hours; 19 were from 24 to 36 hours; 27 from 36 to 48 hours; 21 from 48 to 60 hours; 10 from 60 to 72 hours; and 1 was 90 hours ill.

Whilst it is requisite to correct our arithmetical results and tables to this extent, it will, at the same time, at once be seen that the required deduction is so slight as not in any material degree to interfere with the essential evidence of our various propositions.

CHAPTER II.—THE DANGERS AND ACCIDENTS FROM PARTURITION AND ITS RESULTS ARE GREATER TO THE CHILD IN MALE THAN IN FEMALE BIRTHS.

The increased danger to infantile life dependant upon the sex at birth, may be demonstrated by a reference to the sex of the children in those cases in which the mothers die from labour or its consequences. It may be proved still more strongly and easily, by an appeal simply to the sexes of those children that are dead at the moment of birth, (and without any regard to the fate of the mother;) and by the consideration of the comparative number of accidents and deaths among male and female infants, consequent upon delivery.

We proceed to illustrate each of these points in the form of two or three propositions and their proofs.

Third Proposition.

Amongst the children of the mothers that die from labour or its consequences, a larger proportion of those that are still-born are male than female; and, on the contrary, of those that are born alive, a larger proportion are female than male.

This proposition may evidently be considered under two heads. In the first place, we shall show that a greater proportion of male than female children are found among those infants that were brought forth still-born, by mothers who themselves died from labour or its consequences.

During Dr Collins's mastership there occurred, as we have formerly seen, 154 cases in which the mother died after the birth of single children. The 154 mothers produced 105 male and 49 female children.

The result of the mortality of these infants at birth relatively to their sex, may be reduced to the following short tabular form.

Total male births 105; of these born dead 50, *or* 49 *per cent.*
Total female births 49; of these born dead 16, *or* 34 *per cent.*

In the second place, we remark, that, of those children who were born alive by mothers that died from labour or its consequences, a greater proportion were females than males.

Here we must again take into our calculation the number of mothers who died after uniparous births in connection with the absolute number of male and female children which they produced, when the result, as far as regards those children who were born alive, may be stated in the following form :—

Total male births 105 ; of these born alive 55, *or* 52 *per cent.*
Total female births 49 ; of these born alive 33, *or* 67 *per cent.*

The general proposition may be further proved and illustrated by throwing the results we have just given into one such common table as the following:

Sex.	Total number.	Of these dead.	Of these living.	Proportion of dead to living as
Male,	105	50	55	95 to 100
Female,	49	16	33	48 to 100

Or to state it conversely :—

Sex.	Total number	Of these living.	Of these dead.	Proportion of living to dead as
Male,	105	55	50	110 to 100
Female,	49	33	16	206 to 100

We thus observe, that, amongst the children of mothers dying after uniparous labours, males are born *dead* in the ratio of 95 to 100 living, while with females the ratio amounts only to 48 dead to 100 living. But we find, on the other hand, under the same circumstances, that males are born *living* in the ratio of 110 to 100 dead, while among females the average amounts to the ratio of 206 living to 100 dead.*

Fourth Proposition.

Of still-born children a larger proportion are male than female.

It has already been stated, that, including the whole births in the Dublin Hospital, the male children born were, to the female, in the ratio of 106 to 100. By extended statistical returns, a similar observation has been proved to hold good as a general law in human reproduction, at least among European females.† M.

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* It was not till after the preceding calculations and remarks had been written out that I met with the following confirmatory statement in Dr Clarke's able letters to Dr Price. (See Philosophical Transactions for 1786, p. 349.) Speaking of the maternal mortality in the Dublin Lying-in Hospital from 1757 to 1784, Dr Clarke observes—" I found that of 214 women dead of single children, 50 were delivered of still-born males, and 15 of still-born females ; 76 of living males, and 73 of living females." Hence, calculating upon these data of Dr Clarke, it would appear that, during the above period, male children were born dead in the Dublin Hospital in the ratio of 66 to 100 living, while with female children the ratio amounts only to 20 dead to 100 living ; and, on the other hand, the males were born living in the ratio of 152 to 100 dead, whilst the females were born living in the much higher ratio of 487 to 100 dead.

† For an exception among the free population residing at the Cape of Good Hope see Hawkins' Medical Statistics, p. 51. It is not unworthy of remark, that, among twin children, there seems to be more females than males born. In the Edinburgh Medical and Surgical Journal for January 1844, p. 113, I have reported the sexes of the children in 788 twin cases. Among these there were 756 males and 820 females, or the males were to the females in the proportion of 92 boys to 100 girls.

Bickes, whose results are founded on seventy millions of observations, has shown the proportion of male births to female births to vary in European nations from 104 to 108 boys for every 200 girls.*

All writers on human statistics seem further to acknowledge, that, among still-born children, males occur in a much greater ratio than females. This ratio exceeds very considerably the normal disproportion between the sexes at birth, and is therefore not explicable by it.

The excess of male still-births and the different degrees of it in different returns may perhaps be exhibited most simply in the form of a table, such as we have used in speaking on this subject in our lectures, and which we here insert.

Locality.	Proportion, among Still-born Children, of Males to Females.	*Reporters.*
In Amsterdam,	120 to 100	*Lobatto.*
Geneva,	125 — 100	*Mallet.*
Wurtemburg,	127 — 100	*Riecke.*
Prussia,	135 — 100	*Hoffman.*
Halle,	140 — 100	*Guete.*
London,	140 — 100	*Bland.*
Berlin,	142 — 100	*Süssmilch.*

During Dr Collins's superintendence of the Dublin Hospital, 1121 children were born dead. The number of males to females among these 1121 children stood as follows:

Still-born males	Still-born females.	Or in proportion of males to females as
614	507	122 to 100.

In Dr Collins's reports, the over-proportion of still-born males, though strongly marked, is not so great as in some of the other returns we have quoted. We believe that probably the variation in this respect among the evidence adduced in the preceding table, may be partially explained by the supposition, that, in some of the returns, premature children, and perhaps in others those that were putrid at birth, have not been included. At all events, whenever these are omitted, the number of still-born males in proportion to females will be found to increase. In other words, if we limit our calculations to infants still-born at the full time, and if we consider only those that die during the process of labour, we shall find among them the ratio of males to females to be greatly higher than the average is among still-born children taken indiscriminately, and without reference to the time and cause of their death.

We shall devote our next proposition to the proof of this.

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* Die Bewegung der Bevölkerung mehrerer Europ. Staaten.

Fifth Proposition.

Of the children that die during the actual progress of parturition, the number of males is much greater than the number of females.

It will easily be seen that the data required in proof of this proposition are very difficult to be obtained. Indeed, they could only be accurately ascertained by a series of minute observations instituted through the medium of the stethoscope. This has not yet been done, as far as we know, by any observer. From the want of such evidence, we are under the necessity of approaching as nearly as possible the proper proof, by taking advantage of a mark entered in the elaborate tables of Dr Collins. He has placed a note of "p," (signifying putrid,) opposite to all those children that were born in that state. These "putrid" children had evidently all, or almost all,* died from causes that had operated during intra-uterine life, and before the commencement of parturition. We may, therefore, properly reject such infants from our present calculations, inasmuch as these calculations refer only to children dying *during* the continuance and progress of the parturient process. Of the 1121 children that were still-born, as many as 527 were found putrid at birth. The sexes of the remaining 594 stand in the relation shown by the following table.

Still-born children not putrid.	No. of Males.	No. of Females.	Proportion of males to females as
594	357	237	151 to 100

In the general rate of infantile mortality at birth, we have already seen, under the previous proposition, that in Dr Collins's cases the proportion amounted to about 122 males for every 100 females. In the present instance, and when the children that had died some time before labour began, are excluded, the ratio rises as high as 150 boys to every 100 still-born girls, or as 3 to 2. We might further, from the 594, exclude 62 premature still-born children that were not putrid, (viz. 37 males and 25 females) and retain only those that had reached the full time. This, however, would not alter our results above 1 in the column of 151 males.

But the ratio of these male still-births would probably have been made somewhat greater, and our present proposition still more strongly borne out, had we been able, on the one hand, to take into the calculation those children only who died subsequent to the commencement of the process of parturition, and to exclude, on the other hand, all those that perished, however shortly before, under the influence of other morbific agents than the effects of labour. The fact of the still-born children being "putrid," shows generally that death has occurred several days at least before birth. It is a condition which has enabled us therefore to exclude most, but not all, of those who had died antecedently to the commencement of actual labour. I repeat that, if we had possessed the power of excluding all—and thus so far improving our data—the proportion of male to female children who died during labour (high as we have shown it to be,) would, in all probability, have been found raised still higher.

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* For two remarkable exceptions see Dr Collins's treatise, p. 470 and 483, cases 461 and 1058. In a paper on Peritonitis in the Fœtus, (Edin. Med. and Surg. Journal, Vol. L, p. 390,) I have attempted to show that a large number of those children that are still-born and putrid have died of peritoneal inflammation.

Sixth Proposition.

Of those children that are born alive, more males than females are seen to suffer from the morbid states and injuries resulting from parturition.

Among the accidents and injuries to the child, more immediately resulting from parturition, no one has, of late years, been made the subject of such minute pathological investigation as the "bloody tumour" or "ecchymosis" of the head,—the *Cephalæmatoma neonatorum* of Naegele, Valleix, Dubois, Walshe, and various other modern authors. "The unequal pressure," says Velpeau, "which the cranium experiences, and the tendency that its bones have to over-ride one another, during most labours, when the head is traversing the pelvis, and whether the labour be natural or artificial, is incontestibly the most common cause of this tumour." (*Traité Complet des Accouchmens,* Tom. ii. p. 594.) No author, that I am acquainted with, has given any statistics regarding the sexes of the infants affected with cephalomatomata, with the exception of Burchard. The data which he adduces are not so extensive as could be wished, but still they furnish strong and indubitable proof of our present position. In his essay, "De tumore cranii recens-natorum sanguineo," he mentions that out of 43 cases, which he had observed at the Breslaw Hospital, the child was male in 34 and female in 9 of the instances, so that the relative frequency of the disease in the two sexes stood thus:—

Male Children.	Female Children.	Proportion of Males to Females.
34	9	377 to 100

I am not aware that there has been hitherto published such data regarding serous swellings of the infant's scalp (the *caput* *succedaneum* of the older authors), or the asphyxia or apoplexy of children at birth, or any of the other accidents to which they are subjected during labour, as would enable us to make similar calculations with respect to them. As they are all confessedly more or less direct results of the physical pressure which the infant's head suffers during parturition, the calculations, derived from one, would probably apply with greater or less force to each of the others. Since additional data from *individual* morbid states and injuries are thus wanting in support of the proposition we have laid down, we shall appeal, in confirmation of it, to some calculations we have made from Dr Collins's data regarding the *general* mortality observed among children immediately after delivery. In his chapter on " Children dying in the Hospital," Dr Collins observes, " thirty-two children born at the full period died a few minutes after birth ; in six of these respiration could not be established, though the heart's action continued for some time." (P. 501.) Dr Collins does not offer any clue by which these 32 cases can be discovered in the table attached to this chapter, and thus their sexes ascertained. I find, however, in that table, that 17 single children, born at the full period, are marked as dying within the first half-hour after birth. They hence probably perished from morbid states the more immediate results of labour. Of the 17 children, 9 died within five minutes after birth ; 1 in 10 minutes ; 4 in fifteen minutes ; and 3 in thirty minutes. One of the 17 children was female, and 16 of them males. If we might venture to make the same form of calculation, as we have hitherto used, of the respective sexes of the children from these very small and insufficient data, it would stand thus :

Male Children.	Female Children.	Proportion of Males to Females as
16	1	1600 to 100

Seventh Proposition.

More male than female children die in the earliest periods of infancy ; and the disproportion between the mortality of the two sexes gradually diminishes from birth onwards till some time subsequently to it.

This proposition follows as almost a necessary corollary from those that have preceded it. If, in consequence of the pressure and greater injuries to which male children are subjected during birth, more male than female infants perish during labour ; and if, again, among those born alive more males than females are found to suffer under such morbid states as are the immediate results of parturition, it might justly be *a priori* expected that the same causes which produced these results during delivery and immediately after it, would so far *continue* to affect the male constitution, for some time subsequently, as to predispose it more to disease, and likewise render the diseases which did occur in it more dangerous and fatal in their course than those that affect the female. Further, if this greater liability to morbid action, and its greater intensity and fatality in the male, as compared with the female infant, were the consequence of the male being subjected to greater physical injuries at the time of parturition, the pathological characteristics in question should be observed to diminish more and more in the male system from the moment of birth onwards, because the morbific effects, resulting from any cause or causes operating during birth, would thus progressively diminish, and ultimately pass away. At last, therefore, at some date in early infantile life, the mortality among male and female children, though very different at first, should become nearly or entirely equal. And such, indeed, is the actual state of facts, when the mortality during infancy is investigated upon a large scale. Thus in his observations regarding the " influence of sexes" upon mortality, Quetelet, in his elaborate " Treatise on Man," remarks, as a matter of statistics, that among male as compared with female children, " the ratio of deaths before [during] birth is as 3 males to 2 females; during the first two months after birth the ratio is as 4 to 3; during the third, fourth, and fifth months as 5 to 4; and (he adds) after the eighth or tenth month a difference scarcely exists."*

I shall endeavour to bring forward some statistical evidence in support of the preceding statements.

Dr Collins has given in his Treatise, (p. 519, &c.), a table containing a variety of particulars regarding 284 children that died in the Dublin Hospital within a few days after their birth. Excluding twins and premature children, the date of the death of 148 of these infants is noted in the table referred to. Six of them died on the 8th day after birth; two on the 9th; and one on the 10th. These nine included 5 boys and 4 girls. With regard to the remaining 139 infants that died during the first seven days after birth, the following table, which has been compiled with considerable care, will show the periods of their demise, and the relative proportion which the mortality among the male and female children presented at different dates during the first week of life.†

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* Treatise on Man, Chambers's English edition, p. 50.

† The time the mothers remained in the institution in most instances was for " a period of eight, nine, or ten days after delivery."—P. 500 of Dr Collins' Treatise.

Period of Death.	Males.	Females.	Proportion of males to females.	*Ratio of excess of male mortality.*
Within first half hour,	16	1	1600 to 100	1500
Within first hour,	19	2	950 to 100	850
Within first 6 hours,	29	7	414 to 100	314
Within first 12 hours,	34	15	226 to 100	126
Within first 18 hours,	36	19	191 to 100	91
Within first day,	49	28	175 to 100	75
Within first week,	80	59	136 to 100	36

The last column in the above table shows in the most striking manner both the great proportion of male over female deaths in the first few hours and days of extra-uterine existence, and the rapid diminution which takes place from the moment of birth onward in the relative superabundance of the male over the female mortality. The principal and strong objection that may be urged against the table is the small number of data which is made in it the basis of such interesting statistical deductions. I have given it, however, such as it stands, in consequence of the want, as far as I know, of any more numerous or complete series of similar facts bearing upon the relative male and female mortality in the first week of life. Against the table which I have next to bring forward the same objection cannot be urged, as the individual data are sufficiently ample, and yet the results are exactly the same in their nature, and equally confirmatory of the proposition we are discussing. For the data upon which I have constructed the table, I am indebted to an elaborate extract of deaths at different ages, contained in one of the invaluable reports of the Registrar-General of England. At pp. 144 and 145 of his Fifth Annual Report, (the last published), are given, in two separate tables, the numbers, ages, and sexes of all the individuals that died in England and Wales in the year 1841. In that year 134,563 died who had not passed the age of five; namely, 71,595 males and 62,968 females. In the following table I have arranged these data regarding the male and female deaths, and made calculations from them of such a kind as bear upon the objects of our present proposition

Total number and relative proportion of male and female deaths, during the year 1841, *in England and Wales, within the first five years of life.*

Ages.	Males.	Females.	Proportion of males to females.	*Ratio of excess of male mortality.*
0 to 1 month,	13,351	9,741	137 to 100	37
1 to 2 months,	4,858	3,703	131 to 100	31
2 to 3 months,	3,313	2,676	124 to 100	24
3 to 6 months,	8,008	6,451	122 to 100	22
6 to 9 months,	6,341	5,182	110 to 100	10
9 to 12 months,	5,573	5,013	105 to 100	5
1 to 2 years,	13,987	13,281	100 to 100	0
[*]2 to 5 years,	16,164	15,941	101 to 100	1

A different arrangement of the facts included in the preceding table may probably show still more strikingly the great proportionate male mortality in the period immediately following birth. During the second year of life, that is, from the twelfth to the twenty-fourth month, 13,987 males and 13,281 females died, or nearly an equal number of the two sexes, the excess on the part of the males amounting only to about 700. During the first month of life, that is, from birth up to the twenty-ninth day following it, there died of male infants a number nearly similar to those that perished in the whole course of the second year, namely, 13,351. But the corresponding female mortality during the same period was very much less, amounting only to 9741. Whilst the difference, therefore, between the mortality of the two sexes, during the second year, did not, in the numbers given, show an excess of much more than 700 on the side of the males, this excess ran to upwards of 3500 during the single first month of life, or was five times greater in amount. A tabular arrangement of these figures will point out this contrast more plainly.

Deaths in England in 1841.	Of male children.	Of female children.	*Excess of male mortality.*
During first month of life,	13,351	9,741	3610
During second year of life,	13,987	13,281	700

If it were necessary, and our space permitted, it would be easy to adduce additional tables, constructed from the Registrar's Returns for other years for England and Wales, and from the reports of the mortality in other countries, showing, in correspondence with our proposition, the two facts, 1*st*, that the male mortality, as compared with the female, is much greater immediately after births, and 2*d*, that the male excess of deaths gradually and regularly diminishes from that time onwards, to about the end of the first year of life. The only new and remarkable circumstance that would be proved by these additional tables is, that the rate of the decrease of the male mortality is every where nearly the same.

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[*] In England the proportion of living males and living females does not become equal till about the twentieth year of life. After that period, an excess of the living population is female; before it, a small excess of them is of the male sex, there being originally born from 105 males for every 100 females.—(Fourth Report, p. 12.) Hence, other circumstances being equal, a slightly greater number of males than of females should necessarily be found in the mortality bills in early life, as seen in the last two lines of the table.—(See the Registrar's Fifth Report, p. 23).

In illustration of this last remark, I shall content myself with giving the male as compared with the female infantile mortality in England during 1841, and in Western Flanders from 1827–30. The Flanders mortality is calculated upon a return of 37,203 deaths for the first five years of life, including 21,198 males and 16,005 females.*

Ages.	Male mortality in England.	Male mortality in Flanders.	Female mortality.
0 to 1 month,	137	139	100
1 to 2 months,	131	128	100
2 to 3 ...	124	126	100
3 to 6 ...	122	122	100
†6 to 9 ...	110	117	100
†9 to 12 ...	105	104	100
1 to 2 years,	100	103	100
2 to 5 ...	101	99	100

CHAPTER III.—CAUSES OF THE GREATER MATERNAL AND INFANTILE DANGER AND FATALITY ACCOMPANYING THE BIRTH OF MALE CHILDREN.

On this point Dr Clarke, in his excellent "Observations on some Causes of the Excess of the Mortality of Males above that of Females," published in the Philosophical Transactions for 1786, justly remarks, it may be safely asserted, "that anatomy has not hitherto detected any internal difference between the animal economy of the male and female, which can be supposed to account for their difference of mortality, more especially in early infancy," p. 352. The same author then suggests two sets of causes in explanation of the higher mortality among male children, namely, *first*, their larger size and consequent more difficult delivery; and

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* See Quetelet on the Natural History of Man, p. 30. The town and country mortality are given separately by Quetelet. I have added both together, and computed the proportions in the table from their conjoined sums.

† The dates of the two original returns, from the 6th to the 12th month, do not precisely correspond. In the English tables the returns are from the 6th to the 9th month, and from the 9th to the 12th; in those of Flanders the corresponding returns here calculated upon are from the 6th to the 8th month (not the 9th) and from the 8th to the 12th. The 8th month is not given separately in the published returns, for if so, we would have been enabled to equalize the two exactly.

secondly, their greater liability during their intra-uterine life to disease and debility, from their requiring, in consequence of their size, more actual nourishment from the mother than smaller female children stand in need of. We shall discuss this latter opinion before attempting to show the greater truth and correctness of the former one.

"As the stamina of the male are naturally," (says Dr Clarke,) "constituted to grow of a greater size, a greater supply of nourishment *in utero* will be necessary to his growth than to that of the female. Defects in this particular (nourishment), proceeding from delicacy of constitution or diseases of the mother, must, of course, be more injurious to the male sex."*

"It appears beyond doubt," (says Quetelet,) "that there is a particular cause of mortality which attacks male children by preference, before and immediately after their birth." "It will be interesting," (he further observes,) "to investigate the causes of a circumstance which is so unfavourable to the male sex." "If," (he continues,) "we were desirous of guessing at this point, we might say with those who suppose that a male conception requires a certain excess of energy in the woman, that this excess of energy was absent or wanting during the growth of the fœtus, and, that energy failing, the child would suffer more from it if a boy than if a girl. Hence the disproportion of dead births between the sexes," &c.†

In a review of Dr Collins' Treatise in the Dublin Medical Journal, Dr Graves makes several remarks on the same subject. After showing that Quetelet and Caspar had found respectively in Flanders and Prussia the number of still-born males to still-born females to be 140 to 100, and that in Paris the ratio was 122 to 100, Dr Graves adds, "In Dublin, Dr Collins' numbers give the proportion very nearly the same as that of Paris, so that here we have the additional fact corroborative of those already brought forward, to prove that *a greater* mortality before birth prevails among males than females,—a most curious result well calculated to puzzle both physiologists and philosophers."‡

I shall not attempt to solve, as Clarke and Quetelet have done, the problem broached by Dr Graves, but will content myself with showing, that the observations on which it is founded are in themselves erroneous, and that the tables of Dr Collins (appealed to by Dr Graves) prove, when studied minutely, that, contrary to the general opinion entertained on this point, a greater mortality does *not* prevail before birth among male than female infants. With this view, we bring forward the following as our

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* Philosophical Transactions, Vol. lxxvi. p. 353.

† Treatise on Man, English edition, pp. 25 and 30.

‡ Dublin Journal of Medical Science, Vol. viii. p. 518–9.

Eighth Proposition.

Of the children that die in utero, and before the commencement of labour, as large a proportion are female as male.

I have already had occasion to state, that, among the 1121 still-born children so often already referred to as occurring during Dr Collins' mastership, 527 were putrid, and hence had perished before the supervention of parturition.

If we contrast together the proportionate number of males and females among these 527 children; as well as the sexes of the infants among them that were born at the full time, but putrid; and the sexes of all those that were still-born prematurely, whether putrid or not, we shall find the result to be as follows:—

State of the Children.	Total cases.	Male.	Female	Proportion of Male to Female.
No. of still-born putrid children,	527	257	270	95 — 100
No. of still-born at full time & putrid,	296	148	148	100 — 100
No. of premature still-births,	293	146	147	100 — 100

This table seems particularly valuable and instructive in one respect. It demonstrates satisfactorily, that the intra-uterine morbific agencies (whatever they may be) which act fatally on the fœtus before birth, act equally on the female as on the male child; and that it is to other agencies than these that we are to look for the remarkable proportion of male over female deaths which is observable among still births.

In premature births the comparative compressibility and flexibility of the bones of the cranium is so decidedly greater as to offer no such marked difference between the size and resistance of the male and female head, as we shall immediately see, to exist at the full time.

Now the last column of the table shows, that, in this class of still-births (the premature), the number of dead-born male and female children was exactly equal. The second column testifies, that the same fact holds true of children born at the full time, and in a state of putridity. Further, in looking to the evidence afforded by the first column, we may there observe, that of *all* the children that had perished from intra-uterine causes, and before the commencement of labour (as demonstrated by their putrid state), the females were even more numerous than the males.

The whole series of facts proves (contrary to what is generally alleged), that the proportion of male children that die *before* birth is not greater than the proportion of females. Indeed, if we deducted the usual 6 per cent. for the normal over-proportion of males, the ratio of girls dying before parturition would be found to be greater than that of boys,—a conclusion which the first column would seem to go far to corroborate and strengthen, for in that computation the number of the dead females distinctly and considerably exceeds that of the dead males.

This result, with regard to the equality of the sexes among putrid and premature still-born children, becomes only the more striking when we couple and contrast it with the fact which we have already brought out, that of the children who are still-born, and *not* putrid, as many as two out of every three are boys. In other words, among the infants that die *before* labour, the females are equal, if not greater, in number than the males. Among the infants that die *during* labour the males are raised to the high proportion of 150 boys for every 100 girls. For this sudden and surprising increase, during labour, of the male infantile mortality over the female, there must be some cause or causes traceable in the conditions of the process of parturition. Let us now see, in accordance with what we have already proposed, whether the cause or causes in question be referrible to the greater size of the male infant, and the effects of this size upon the compression of the cranium and its contents. And in order to understand more thoroughly the investigation, let us inquire, in the first place, into the actual average difference between the volume and weight of the male and female at birth.

RELATIVE WEIGHT AND SIZE OF THE MALE AND FEMALE AT BIRTH.

In the Dublin Hospital, Dr Clarke long ago endeavoured to discover the relative weights of the new-born male and female child, by observations made upon 60 children of each sex. In his paper in the Philosophical Transactions of 1786, (to which we have repeatedly referred), he gives both the absolute and average weights of these 60 male and 60 female infants.

The 60 males weighed in all 442 lbs.*

The 60 females weighed in all 404¼ lbs.

The average weight of the male was 7 lbs. 5 oz. 2 dr.

The average weight of the female was 6 lbs. 11 oz. 2 dr.

The average difference between the weight of the male and female child, as calculated from these 120 instances, thus amounted to about nine ounces.

In the Edinburgh Lying-in Hospital, 50 male and 50 female children, born during the latter months of 1842 and the earlier part of 1843, were weighed by my friend and assistant, Dr Johnstone.

The 50 males weighed in all 383 lbs. 11 oz. 4 dr.

The 50 females weighed in all 342 lbs. 12 oz. 4 dr.

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* The Troy or Apothecaries' weight is there used.

The average weight of the male was 7 lbs. 9 oz. 1 dr.

The average weight of the female was 6 lbs. 12 oz.

The difference between the weight of the male and female child, as calculated from these 100 cases, thus amounted to about ten ounces on the average.

The respective lengths of these 50 male and 50 female children were also carefully ascertained in the Edinburgh Hospital.

The total length of the 50 males was 1020½ inches.

The total length of the 50 females was 990½ inches.

The average length of the male child was 20 inches 5 lines.

The average length of the female child was 19 inches 10 lines.

The average difference between the length of the 50 males and 50 females thus amounted to 7 lines, or somewhat upwards of half an inch.

These results are sufficient to prove, that at birth, as at other subsequent periods of life, the male is usually of a greater weight and size than the female. The general volume, however, of the *body* of the child is not, in relation to the mechanism of parturition, a matter of such immediate importance as the size of the *head* itself, the facility or difficulty of the progress being principally dependant upon the relative size of the latter. The greater weight and volume of the male than the female child at birth might, *a priori*, entitle us to calculate that the head of the male would, in correspondence with the other parts of the body, be larger than the head of the female. In such an inquiry, however, as the present, it is better to refer to direct arithmetical facts than indirect though probable inferences, and Dr Clarke has left us a series of measurements of the heads of male and female children, at birth, that are valuable in giving us more precise ideas upon this point. His observations were made upon the 60 male and female children whose respective weights we have already detailed. "For measuring their heads, I made use," he observes, "of a piece of painted or varnished linen tape, divided into inches, halves, and quarters. I took *first* the greatest circumference of the head from the most prominent part of the occiput around the frontal sinuses; and, *secondly*, the transverse dimensions from the superior and anterior part of one ear across the fontanelle to a similar part of the other ear." These data appeared to me the most likely to afford data for determining the respective sizes of the brain in the different sexes." (P. 358).

The result of Dr Clarke's measurements may be exhibited in the following manner.

Since these remarks were sent to press, I have incidentally met with the following remark in Dr Forbes's Quarterly Medical Review, Vol. x. p. 492. "M. Nevermann gives us the results of the measurements of the heads of 384 children by Professor Thulstrup of Christiana, which fully bear out Dr Clarke's statements."

	Absolute dimensions in		Average dimensions in			
	60 males.	6) females.	60 males.		60 females.	
	Inches.	Inches.	Inches.	Lines.	Inches.	Lines.
Circumference of head,	839	817	13	$11\frac{4}{5}$	13	$7\frac{2}{5}$
Dimensions from ear to ear,	$445\frac{3}{4}$	$433\frac{1}{4}$	7	$5\frac{1}{7}$	7	$2\frac{3}{4}$

The difference brought out in the preceding table, between the male and female head, may appear more precise if we reduce them to decimal figures.

	Average circumference of head.	Average dimensions from ear to ear.
	Inches.	Inches.
In male child,	13.983	7.429
In female child,	13.617	7.221
Difference,	0.366	0.208

According to these observations upon new-born children it would appear that,

1. The head of the male infant, when measured across from ear to ear, over the fontanelle, is about $2\frac{5}{7}$ lines, or nearly two-eighths of an inch greater than that of the female.

2. In circumference, the head of the male is $4\frac{2}{5}$ lines, or almost precisely three-eighths of an inch greater than that of the female. Hence,

3. The *transverse* diameter of the male head is nearly one-eighth of an inch greater than the transverse diameter of the head of the female child.*

The preceding difference between the absolute size and weight of the male and of the female child at birth, and between the relative dimensions of the male and female infantile head, may appear to some to be on the whole so slight, as not to afford in themselves any sufficient explanation of the differences that we have seen to exist, between the dangers and mortality incident respectively to male and female births. The general greater size of the head and body of the boy at birth may not seem adequately to account, by any influence traceable to it alone, for the general greater peril and fatality accompanying the birth of male as compared with female children. The alleged cause may appear unequal to the production of the alleged effects. Let us state, therefore, the reasons that induce us to hold the contrary opinion, and which impress us with the belief that the difference between the size of the male and female head at birth (inconsiderable as it may seem) is the true cause of the greater danger and fatality to mother and child, which accompanies the parturition of male infants.

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* Calculating upon the accuracy of Dr Clarke's linear measurements of the fœtal head, it would appear that the *surface* of the cranium of the male infant, (*above* the circumferential line of measurement,) is about 27.8 square inches; that of the female about 26.3 square inches. The arch of the male cranium, at birth, is therefore, superficially, upwards of one square inch greater than that of the female. To state it in other words, the proportion of the surface of the head of the male new-born child to that of the female is nearly as 19 to 18,—or the surface of the head of the female is one-nineteenth part less than that of the male.

Before doing so it may be proper to premise, that under our fifth and eighth propositions I have already shown, on the one hand, the explanation of the cause of the greater number of male than female still-born infants proposed by Clarke, Quetelet, &c., as depending upon intra-uterine agencies, is totally incorrect, and disproved by statistical facts; and, on the other hand, we have seen that the increased mortality of children of the one sex over the other at the time of birth is a result of some circumstance or circumstances connected with, or at least only in operation during the process of labour. Whilst this holds true as regards the child, it is of consequence, at the same time, to recollect that the complications and dangers on the part of the mother, which we have found so much more frequently attendant upon male than female births, are all of such a nature as to be confessedly the direct or indirect consequences of causes acting during parturition Further, it will, we imagine, be granted by every accoucheur, that whatever circumstance or circumstances may, by operating as a cause of obstruction, lead to the greater danger and fatality of the male infant during the progress of labour, will, at the same time, in all probability, equally explain that greater peril and fatality to the mother during the process, which is incident to the births of males.

For the solution of the present problem, both as regards the fate of the mother and of the infant, we believe that we ought to look alone to the greater size of the head of the male infant, and that for a variety of reasons which we shall now endeavour to state *seriatim*, adding such illustrative remarks and evidence as they may seem to require, and throwing the more important deductions that may seem to flow from the statistical proofs brought forward into the shape of additional propositions.

REASONS FOR CONSIDERING THE GREATER SIZE OF THE HEAD OF THE MALE CHILD AS THE CAUSE OF THE GREATER NUMBER OF COMPLICATIONS AND CASUALTIES ACCOMPANYING MALE BIRTHS.

We shall begin our enumeration of these reasons by stating that, *First, For the very marked differences existing between the difficulties and perils of male as compared with female births, there is no other traceable cause in the mechanism of parturition except the larger size of the head of the male child.*

Parturition, at the completed term of pregnancy, is a process consisting of a combination of mechanism, intended for the extrusion of the full-grown infant from the cavity of the uterus. For the perfect action of this mechanism, three sets of physical conditions are essentially necessary. *First*, A certain degree of mechanical expulsive power is required, and this is supplied principally by the vital contractions of the uterus. *Secondly*, An adequate degree of capacity and dilatation is necessary on the part of those maternal passages through which the child is to be expelled. *Thirdly*, The body which is to be expelled (viz. the infant) must be of such size, and be placed in such a position or positions as to allow of its sufficiently easy entrance and progress through these maternal passages. A perfect and equitable adjustment between these three different conditions is necessary to the constitution of natural labour; and on the other hand, a deviation—absolute or relative—in any one or more of these conditions leads on to tedious and difficult parturition, and its results. In which of these three essential conditions could a deviation be possibly effected by the sex of the child, so as to account for the greater dangers and complications attendant upon male births?

The mere sex of the child, and the circumstance, whether it was a male or female, could evidently produce no primary effect upon the vital muscular contractions of the uterus, so as to alter them in any morbid manner. Consequently we may exclude from our consideration the first set of physical conditions that we have above named.

We may do equally the same with the second, because it is impossible to conceive that the measurements of the pelvis and maternal passages could be directly altered in any way by the child which was to pass through them being male rather than female.

In searching for a possible cause of deviation in the mechanism of parturition, explanatory of the facts which we have traced, we are thus by a method of exclusion compelled to look to the third set of physical conditions of labour, viz. the position and size of the infant.

The consideration of the position of the infant will afford us no clue to the problem, because in most of the cases from which we have drawn our data the presentation and position were natural. When it was otherwise, the deviation was as frequently on the part of the female as of the male child.

The only item, therefore, that is left in which we may trace any distinction whatever between the mechanism of labour in male and in female births is the relative greater size of the infant of the male sex, and this relative greater size could only influence labour or its consequences in as far as it affected the dimensions of the *head* of the child,—the increased volume of its body never, unless when excessive in degree, retarding or complicating to any extent the progress of the labour in cases of cephalic presentations. The difference between the male and female infant's head is indeed slight, but we are forced, at this stage of the inquiry, to believe it to be the probable origin of the greater dangers and more numerous complications accompanying male births, simply in consequence of our being utterly unable to trace any other appreciable difference, or imagine any other possible circumstance, in the conditions of parturition, that could afford the most distant explanation of the series of phenomena that we are discussing. It is the only circumstance which we can detect as an apparent and constant antecedent to the consequences we have described; and hence seems to be that particular condition which we ought to look upon as the cause of these effects.

In the foregoing remarks we have arrived at the conclusion that has just been stated by a kind of reasoning by exclusion. The argument we have been considering is in a great measure negative. But others of a more positive character are not wanting in confirmation of the same view. For,

SECONDLY, *An increase in the effects may be shown to be connected with an increase in the alleged cause.*

"When," observes Dr Hamilton, "the head of the infant is pushed foremost, and the labour is not completed within twenty-four hours from its actual commencement, the case is styled laborious, and it may terminate in one of three ways. Thus, the natural powers may at last complete the delivery; or, though these fail, it may be possible for the practitioner to complete the delivery by mechanical means, with safety both to mother and child; or it may be impossible to draw the infant alive through the natural passages. These *three* several terminations constitute *three different orders of laborious labours*."* (Outlines, p. 45.)

Now, if it were granted us, for the sake of argument alone, that the greater obstacles and perils attendant upon male births are attributable to the larger size of the male infant's head, it would almost necessarily follow, that as in proportion as the difficulties connected with the above three different orders of laborious labour progressively increased over one another in intensity, the amount of male children connected with these labours would, in like manner, probably progressively increase in number.

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* "It is," says Dr Churchill, "peculiar to midwifery operations, [and the same remark applies equally to the different orders of laborious labours,] that they form an ascending series, increasing in gravity from the simplest to the most severe,—no two being equal."—Researches in Operative Midwifery, p. 12.

Let us interrogate the data to be found in Dr Collins' reports in order to test practically the truth of an opinion which is so far merely theoretical.

Under the second proposition, we have had already occasion to state that, during Dr Collins' mastership in the Dublin Lying-in Hospital, among the 16,414 labours which he has reported, the sexes of the infants born were in the proportion of 106 males to 100 females.

Out of 109 tedious and difficult labours, in which Dr Collins has recorded the sex of the infant, and that occurred among these 16,414 deliveries, 65 of the children were male and 44 female.

In 24 forceps cases met with by Dr Collins, the proportion of male to female children was still greater, the males being 16 and the females 8 in number.

The ratio of male infants mounts yet higher, when we turn to the cases in which the labour was so difficult as to require craniotomy. Among 74 instances in which the crotchet was used by Dr Collins, and where the sex of the child is mentioned, the infant was male in 50 instances and female in 24.

A tabular arrangement of the above results will show the proportion of male to female children under these different classes of head presentations.

Nature of the Labour and Complication.	Total Cases.	No. of Male Children.	No. of Female Children	Proport. of Males to Females.	*Ratio of Excess of Males.*
Labours generally,	16,654	8548	8069	106 to 100	6
Tedious labours,	109	65	44	148 to 100	48
Forceps cases,	24	16	8	200 to 100	100
Crotchet cases,	74	50	24	208 to 100	108

The preceding data prominently show, that as the classes of labours increase in severity and difficulty, the proportion of male to female children in them increases in a corresponding degree. Evidence of exactly the same kind, in support of the same opinion, may be gathered from a tabular extract of the cases that occurred in the Dublin Hospital during the mastership of Dr Clarke, (viz. from 1787 to 1793), published by that gentleman in the first volume of the Transactions of the King's and Queen's College of Physicians of Ireland, p. 400. In this abstract Dr Clarke states the sex of those infants that were still-born in the three classes of 1. ordinary, 2. tedious, and 3. laborious labours.

During Dr Clarke's mastership there occurred 10,199 cases of single or uniparous births. Among these, 340 children were still-born though the labour was natural, that is, was terminated within 24 hours, and with the head of the fœtus presenting. Of these 340 still-born children, 170 were male and 170 female. The ratio of the sexes shows this perfect equality in consequence probably of this list including principally putrid and premature children, among whom we have already found (see the eighth *proposition)* the number of still-born boys and girls to be nearly alike, and the over proportion, if any, to be rather on the side of the females.

Under the term "Tedious Natural Labours," Dr Clarke includes those which exceeded 24 hours in duration, but where there was no such disproportion between the head of the fœtus and the mother's pelvis, as to render *destructive* instruments necessary. This class of labours is produced either (he observes, p. 371) "by causes weakening the expelling powers of the mother, or increasing resistance to the passage of the fœtus." In Dr Clarke's report, 134 cases are referred to this division, including 16 forceps cases. In these 134 labours, the child was still-born in 41 cases. Among these 41 still-born children, 26 were male and 15 female.

Dr Clarke further reports the sex of the child in 48 cases of what he terms "Laborious Natural Labours," and where," (to use his own words,) "the disproportion between the head of the fœtus and the pelvis was so great that it became necessary to diminish the bulk of the former to save the life of the latter." Among these 48 crotchet cases, 32 of the children belonged to the male, and 16 to the female sex.

Of the 429 children that there were still-born, in these different classes of natural or head presentations in Dr Clarke's practice, the proportion of the sexes was therefore in the following ratio :—

Nature of the Labour and Complication.	Total No. of still-born children.	Of these were male	Of these were fem.	Proport. of males to females.	*Ratio of excess of males.*
Ordinary natural labour,	340	170	170	100 to 100	0
Tedious natural labour,	41	26	15	173 to 100	73
Laborious natural labour, (crotchet cases,)	48	32	16	200 to 100	100

The preceding data, from Dr Collins' and Dr Clarke's returns, will be probably admitted to be sufficient to prove the point that we have in view, and to warrant the following inference as a

Tenth Proposition.

In "laborious labours," with the head presenting, in proportion as the order of labour rises in difficulty the amount of male births in them rises in number.

To the proof that I have just given, from the reports of Drs Collins and Clarke, of the truth of this proposition, it gives me much satisfaction to add the following general confirmatory statement contained in an analysis of Riecke's elaborate obstetric statistics of the kingdom of Wurtemberg, published in the Archives Générales.* As one of the results of these statistics, (the most extensive hitherto published in midwifery,†) it is stated that "the proportion of boys to girls is much greater in 'artificial' than in ordinary labours, for it includes 7 boys for every 5 girls, [or 140 male children for every hundred female.] There is the same fact observable among the sexes of those children that are the products of artificial births, and who are either dead born or die shortly afterwards, for among them the proportion of boys to girls is as 8 to 5, [or as 160 male to every 100 female infants.] Often enough, (it is added,) it happens that the same woman cannot, without aid, be delivered of a boy, who, at all her other confinements, when the child was female, required no assistance." p. 88.

THIRDLY, *A diminution of the cause, leads to a diminution in the effects ascribed to it.*

We have alleged the cause of the increased casualties connected with male births to the greater size of the head of the male than of the female infant.

In premature infants there does not exist so great a difference, as in those born at the full time, between the size of the head of the male and female child. The fœtuses of the two sexes approach one another more and more in physical and other characters, the earlier the period at which we compare them. Besides, the cranium of the premature child is so compressible, from its deficiency of ossification, as to be much more easily reduced in size under the pressure to which it is naturally subjected in the process of parturition, and hence the slighter difference which may actually exist between the dimensions of the heads of male and female premature infants, is still more diminished in its effects and operation during the course of labour.

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* Archives Générales de Médecine, Tom. xx. p. 76.

† They include 219,353 deliveries which occurred in Wurtemberg during the four years, 1821–25. The artificial deliveries, (*accouchmens artificielles*,) or those requiring some special aid or interference, amounted to 7949. In these 7949 cases, 630 of the mothers and 3754 of the children were lost. The generalization in the text seems to be founded upon the sex of the infant in these 7949 labours, and 3754 still-births.

Possessing such physical conditions, the birth of premature children should, from the diminished size and diminished resistance offered by their heads, give rise to fewer of those casualties in labour that we have attempted to trace to the influence arising from the greater size of the head of the full grown male child.

I have no evidence to bring forward with a view of showing the influence of the birth of premature children or their sex, on the difficulties of the labour in reference to the safety and life of the mother, because the labours with them are seldom or never so difficult as to induce any marked maternal complications. But we may derive equally good evidence in illustration of the point we are discussing, by studying the effects of the labours with premature children,—not upon the *mothers* but upon the *infants* themselves. With this view I shall collect, from the data contained in Dr Collins' report, the number of male and female children expelled *prematurely* and not putrid, in order to contrast them with the proportion of males and females among still-born children expelled at the *full time* and not putrid. Of the former there were born in all 62, of the latter 532. The ratio of the sexes in each series stands thus :—

Non-putrid Children still-born.	Total.	Males.	Females.	Proportion of Males to Females.
At full time,	532	320	212	151 to 100
Prematurely,	62	34	28	121 to 100

The comparison of the two columns in the above table shows that, among the children who died immediately before or during labour, and who had reached the ninth month of utero-gestation, the proportion of males to females lost was considerably greater than among still-born children born prematurely,—and, as we believe, for this reason, that in the latter, (the premature still-births,) there is not, as we have pointed out, so great a distinction between the male and female heads as among children at the full time. The comparison of the proportion of males to females who died within ten days after delivery among, 1st, children born at the full time, and 2d, those born prematurely, leads to the same inference, viz. that as in the latter the alleged cause of distinction between the relative size of the male and female head is diminished, the alleged effect upon the differences displayed between the mortality of the two sexes is also proportionally lessened. The following table is intended to illustrate this point. The calculations are from the data in Dr Collins' chapter on children dying in the hospital.

Children dying within 10 days after birth.	Total.	Males.	Females.	Proportion of males to females.
Born at full time,	171	106	65	163 to 100
Born prematurely,	102	56	46	121 to 100

The remarks that we have made regarding the heads of premature children should apply equally to those of twins. Twin children, like those born prematurely, are in general below the standard size and weight, and, in the same manner, have their heads less ossified, and hence are more compressible than those of single infants at the full time. There is, consequently, also a less marked difference between the relative size of the male and female head, among twin children, than among single children at the full time; and for the same reason, if our views are true, there should be a similar less marked difference between the mortality of the two sexes among twins, at birth and for some time after it. That such is in reality the case I shall allow Dr Clarke to state in his own words. Speaking of the relative male and female infantile mortality in the Dublin Hospital, he observes, (Philosophical Transactions, Vol. 76, p. 354,) "it is worthy of observation that though *double* the numbers of twins die and are still-born, compared to single children, yet the proportion of male twins lost to females is *less*. Only one-fifth more of the male sex die than of the female, and only one-third more is still-born. Whereas of single children, whose proportional mortality is one-half less, *one-fourth* more of the male sex die, and near double the number is still-born. To what, then, are we to attribute this lessened mortality in favour of male twins? Probably to their brain and nervous system suffering less during delivery, on account of their heads being much smaller than those of single children."* In other words, the cause of the higher mortality, or the relative size of the male over the female head, being diminished, the effects which we attribute to it, viz. the higher infantile mortality among male children, is also proportionally diminished.

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* In the Dublin Hospital, from the year 1757 to 1784, it appears from Dr Clarke's tables, that among the twin-births 29 males and 20 females were still-born, and 116 male and 91 female children died within a fortnight after delivery; on the

FOURTHLY, *In those morbid complications in labour in which the cause is in abeyance, the effect is also absent.*

There are some morbid complications during labour which are independent of the presence, and hence of the sex or size of the infant. I allude especially to those complications which occasionally occur during the third stage of labour, and consequently after the total expulsion of the child. It would afford no small corroboration of the preceding remarks and inferences, if in these cases (where all agency from the conditions of the infant was for the time being excluded,) the morbid complications which may take place, were found to occur as frequently in connection with the birth of female as of male infants. And such, we believe, may be shown to be the fact.

The two principal complications that occur during the third stage of labour are morbid retention of the placenta and hæmorrhage.

Dr Collins reports 70 cases of retention of the placenta, 35 of them after male, and 35 after female births.

Hæmorrhage took place during the third stage of labour, or between the birth of the child and placenta, in 71 cases during Dr Collins' mastership. In 36 of these 71 labours the child was of the male, and in 35 of the female sex.

These observations, when thrown into a tabular form, would consequently stand thus:—

Nature of complication.	Total cases.	With male children.	With female children.	Proportion of males to females.
Retention of placenta,	70	35	35	100 to 100
Hæmorrhage during third stage,	71	36	35	100 to 100

The evidence afforded by the above table is probably sufficient to entitle us to add to those inferences that we have already laid down, the following deduction as a

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other hand, among the uniparous or single births 602 males and 351 females were still-born: and 1656 males and 1247 female children died within a fortnight after birth. These data form the ground of the inferences drawn by Dr Clarke in the passage quoted in the text.

Ninth Proposition.

Of the morbid accidents that are liable to happen in connection with the third stage of labour, as many take place with female as with male births.

To prove that the slight increase of size of the male over the female infant's head is the true antecedence or cause of the greater number of casualties accompanying male births, we may so far change the ground of direct proof which we have been hitherto attempting to pursue, and proceed to show, as a matter of strong though indirect evidence, that,

FIFTHLY, *Similar effects upon the mother and child are produced by other causes similar in their character and amount.*

If the larger dimensions of the head of the male infant be the immediate cause of the larger number of accidents and deaths attendant upon male births, its greater degree of ***size*** could only possibly lead to the results in question, by offering a greater proportionate degree of impediment to the passage of the fœtus through the pelvic canals. Its effects, therefore, upon the mother and child should be such as are produced by obstructed labour. That the various consequences which we have previously traced are all of that description, it is unnecessary for us to point out to the obstetric pathologist. It might be considered, however, as confirmatory of this fact, and, at the same time, as the strongest correlative evidence that could be adduced of the power of ***slight*** obstructions, during labour, to lead to these effects, if we could prove that in any other extended series of cases, in which obstructions of the same amount existed, the same or similar effects resulted both to the mother and child. Our evidence would only be the stronger if, in this class of cases, we could so far reverse the state of matters as to transfer the existing slight obstruction from the body passing to the passages themselves. The circumstances connected with first labours offer the exact conditions which we seek.

In first labours, taken as a class, there is a greater obstruction to the transit of the child than in subsequent deliveries, in consequence of the maternal passages being less dilatable than afterwards. The two obstructions, viz. the increased size of the fœtal head, and the diminished size of the maternal passages, though different in their seat, are nearly analogous in the ***nature*** of their influence, and in the amount of that influence.

In male births, the body passing, (the head of the infant) is slightly *greater* than in female births, whilst, *cæteris paribus*, the maternal passages are the same. In first labours the capacity or dilatability of the maternal passages is slightly *less* than in subsequent labours, whilst, *cæteris paribus*, the head of the infant remains the same, there being at least an equal number amongst them of males and females. Having stated these premises we shall go on to point out, as another reason for supposing the size of the male head to be the sole cause of the danger of male births, that, in accordance with our proposition, most of the complications and accidents, which have been already described as arising in male, as compared with female births, from the comparatively slight increase in the dimensions of the male head, occur also in first, as compared with future labours, in consequence of the comparatively slight diminution of the dimensions or dilatability of the passages in these labours.

We shall give the proofs of this as an illustration of another, or

Tenth Proposition.

More dangers aud deaths occur both to mothers and children in first than in subsequent labours.

Among the 16,414 women delivered in the Dublin Hospital during Dr Collins' seven years mastership, 4987 were confined for the first time. First deliveries occurred, therefore, in the proportion of about 30 in every 100 cases. If, however, in studying the various morbid complications which have been statistically reported by Dr Collins, we find the proportion of these complications in first labours to be greatly above the ratio of 30 per cent., it would tend to show that there is a stronger liability to their occurrence in first than in future deliveries. To gain some data in this question, let us examine into the facts which may be gathered from Dr Collins' returns in regard to the proportion of these morbid accidents arising from obstruction during parturition, which were met with in first as compared with subsequent labours.

Convulsions.—Of 30 cases of convulsions described by Dr Collins, 29 occurred in women who were pregnant for the first time, (page 201.) Thus they were found in connection with first pregnancies in the ratio of 96 in 100.

Crotchet cases.—Of the 79 cases which Dr Collins mentions as requiring craniotomy, the number of the pregnancy in each can be made out only in 75. Of these, 51 were first labours, (Table, page 490, et seq.) ; or out of every 100 cases in which the crotchet was used 68 were first deliveries.

Forceps cases.—The forceps were employed in 24 instances.

Of these, 18 were first labours, (page 15.) Hence out of every 100 instances in which this instrument was used, the proportion of first labours, according to this average, would be 75.

Tedious labours.—Under our second Proposition I have already stated that Dr Collins has given so far the history of 109 instances of tedious labours, (page 462, et seq.) Of the 109 cases, 75 were first confinements, (Table, page 490, et seq.); thus giving for every 100 cases a proportion of 69 as pertaining to first deliveries.

Puerperal fever.—Out of 88 women attacked with puerperal fever, 44 were confined for the first time, (page 284.) Thus this complication supervened upon first labours in the ratio of 50 in every 100 cases.

Maternal deaths.—We have already seen, under the first Proposition, that of the 16,414 women delivered in the Dublin Hospital during Dr Collins' charge of it, 164 died, (page 363.) Of these 164 mothers, 86 had given birth to first children, (page 363); or of every 100 women who died, 53 had been confined for the first time.

Still-births.—Under this head we exclude from our calculation all those children who were born putrid either prematurely or at the full time. Of 1121 still-births in all, observed by Dr Collins, 594 were born not putrid. Of these 594 children, 260 were the product of first confinements; or, in other words, they were found in connection with first pregnancies in the ratio of 45 to 100.

Infantile deaths during the first eight or ten days after birth.—The mothers and their infants generally, (as we have seen,) remained for that time in the hospital after delivery. During it 284 of the children died. Of these, 170 were single children born at the full time, (analysis of table, page 519, et seq.) Of the 170, 75 were the offspring of primiparæ; or infantile deaths, during the first ten days, among single children born at the full time, occurred in connection with first pregnancies in the ratio of 44 in every 100.

The preceding series of facts, if arranged together into a general tabular form, would stand as follows. In reading these arranged results it is to be recollected that the *standard proportion of first pregnancies, in the general sum of the labours, amounts only to* 30 *per cent.*; whilst, as shown by the table, the proportion was much higher in the various complications under mentioned.

Complication.	Total Cases.	1st Pregnancies.	Proportion of 1st pregnancies
Convulsions,	30	29	96 in 100
Forceps cases,	24	18	75 in 100
Crotchet cases,	75	51	68 in 100
Tedious labours,	109	75	69 in 100
Puerperal fever,	88	44	50 in 100
Maternal deaths,	164	86	53 in 100
Still births, not putrid,	594	260	45 in 100
Infantile deaths during 1st 10 days; born single and at full time.	170	75	44 in 100

The results which the above table is intended to display might perhaps be brought out more strongly by another arrangement, if our data for it were more complete. Dr Collins mentions the number out of the 16,414 patients that were delivered for the first time, viz. 4987; of these 72 were twin births. Unfortunately, however, for our present purpose, he has omitted to give the exact numbers of those that were confined in their second, third, and other subsequent labours. But an approximation, sufficiently near for the purpose we have in view, may perhaps be made without any probability of a very serious error. To have the required standard of comparison, I shall take it for granted, (for reasons which it is unnecessary to dwell upon,) that the per centage of first and future labours in the Dublin Hospital was nearly as follows:—Of all the cases, about 30 per cent. were first pregnancies; 22 per cent. second pregnancies; 15 per cent. third pregnancies; 11 per cent. fourth pregnancies; 8 per cent. fifth pregnancies; 5 per cent. sixth pregnancies; 3 per cent. seventh pregnancies; 2 per cent. eighth pregnancies; 1 per cent. ninth pregnancies; and 3 per cent. belong to women who were confined for the tenth time or upwards.* If we compare with this given standard the proportion of instances in which the different complications we have been considering occurred in different pregnancies, we will see in a marked degree the great over-proportion of them in connection with first deliveries. I have constructed the following table with this view.

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* This calculation of per centages proceeds on the assumed probability that, of the 16,414 women delivered in the Dublin Hospital under Dr Collins, 4924 were cases of first labour; 3611 were of second labours; 2462 third labours; 1806 fourth labours; 1313 fifth labours; 826 sixth labours; 495 seventh labours; 328 eighth labours; 164 ninth labours; and 494 were instances in which the women were delivered for a tenth or later time.

Table showing the per centage of cases of natural and morbid labours belonging to different pregnancies.

Per centage of	1st Preg.	2d Preg.	3d Preg.	4th Preg.	5th Preg.	6th Preg.	7th Preg.	8th Preg.	9th Preg.	10th & subseq. Pregs.
Total No. delivered in Hosp.	30	22	15	11	8	5	3	2	1	3
Convulsions,	96	4	0	0	0	0	0	0	0	0
Tedious labours,	69	9	7	3	2	3	2	1	3	1
Forceps cases,	75	?	?	?	?	?	?	?	?	?
Crotchet cases,	68	10	7	4	0	3	?	0	2	4
Puerperal fever,	50	19	10	7	8	0	2	4	0	0
Maternal deaths,	53	16	7	7	5	4	1	3	1	3
Still-births,	45	17	11	8	5	5	4	3	4	8
Infantile deaths for 10 days after birth,	44	15	11	8	8	7	2	3	1	2
Post-partum hæmorrhage,	33	16	10	9	9	5	4	6	3	6
Rupture of uterus,	21	18	18	6	6	15	6	3	3	10

In regard to the two complications which we have placed at the bottom of the above table, viz. *post-partum* hæmorrhage and rupture of the uterus, it will be observed that our present proposition does not hold out so strictly towards them, and that they do not occur comparatively more frequently in first than in subsequent labours. But this does not detract in any way from the previous view which we took of them, as occasional results of the partial obstruction offered by the male head. For there are sufficient reasons why they should occur more rarely in first labours, though these labours are more obstructed than those that occur subsequently. In regard to rupture of the uterus, for example, it is now well known to obstetricians that two causes may give rise to its occurrence, namely, *first*, over-action of the uterus, in consequence of impediment to the passage of the child;* and, *secondly*, any such diseased condition of a portion of the uterine parietes as renders that portion less resistant, and hence more easily lacerated than the other parts of the uterine walls. The first of these two causes is in much stronger operation in male than in female births, and in first than in future labours; but the second cause comes more and more into action with the frequency of the previous parturitions, the uterine tissues being more and more liable to disease under the strain of each successive pregnancy and labour. Hence, though lacerations from obstruction are more common in first confinements, the same accident, as a result of disease of the uterine structures, is far more frequent in subsequent and later parturitions, so much so indeed, as to cancel any argument that might be derived in favour of its origin from mere obstruction, by the study of it in primaparous mothers.

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* "There is one fact," observes Dr Collins, "which clearly shows disproportion to be a frequent cause, namely, its being oftener met with in the expulsion of male children. Thus, of 34 cases which I am about to state, 23 of the children were males, &c. This is satisfactorily accounted for by the greater size of the male head, as proved by accurate measurements made by Dr Joseph Clarke."—Chapter on Rupture of the Uterus and Vagina, in Practical Treatise, p. 242.

Again, *post-partum* hæmorrhage, though more apt, as we have already stated, to occur in cases in which the uterine action has been protracted, and the contractile powers of the organ morbidly exhausted, is not so liable to appear after tedious first labours as after tedious subsequent labours, because it is a well-known and acknowledged fact, that altogether the uterus contracts more perfectly and securely after first labours than after others. Hence the well-known rarity of after-pains subsequently to first parturitions. "A woman," says Dr Power, "experiences little or no after-pain with her first parturition, because the parietes of the uterus, not having been weakened by previous distension, contract more perfectly and permanently, so as to obliterate and empty the cavity thoroughly."*

In reference to the bearing of some of the results included in the above tables upon the question we are discussing, it is proper also to add, that the ratio in which some of the complications occurred in first pregnancies is greater in degree than can be accounted for upon the doctrine of obstruction, and is partially dependant upon other coexisting causes. I allude more particularly to the case of puerperal convulsions. It is well known that in the practice of all accoucheurs this complication is met with, principally in first, and comparatively rarely in future deliveries.† The mere amount of obstruction present in first labours is not the sole explanation of the large percentage of convulsions occurring in first confinements, for impediments in the maternal passages in future deliveries are not followed in any such degree by the same conse-

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* Treatise on Midwifery, p. 190.

† The following table will show this fact better than any lengthened commentary.

Total No. of Puerperal Convulsions.	No. of these occurring in first labours.	Reported by
59	45 or 57 per cent.	F. Ramsbotham
22	15 or 68 —	J. Ramsbotham
46	30 or 65 —	Lee
48	36 or 75 —	Merriman
19	15 or 79 —	Clarke
9	8 or 89 —	Johns
30	29 or 96 —	Collins
233	178 or 76 per cent.	Total cases.

quence; and, further, obstetric pathologists are now well aware that in almost every case of puerperal convulsions, a predisposition to the affection is given by one or more particular pre-existing morbid states of the system. Various authors, as Demanet,* Osiander, Chailly, Johns, &c. have correctly described the most common pre-existing morbid condition as one marked by dropsical effusions in the face and elsewhere. Of late years the pathology of these cases has been advanced a step farther, and the complication of convulsions shown to occur only, or almost only, in women who have such confirmed or temporary renal derangement as is marked by an albuminous state of the urine. Dr Lever suggests that this albuminous state of the urine, (the common state predisposing to puerperal convulsions,) is produced by pressure of the enlarged uterus upon the renal veins. If this were the explanation, this complication should occur as frequently in subsequent as in first labours, because the renal veins are not more specially compressed in the one than in the other. But to discuss any such question at present would be wandering from our subject. We have said enough to show that, in regard to puerperal convulsions and their frequency in first labours, we must take other points into consideration than the mere degree of obstruction, in order to account for their high over-proportion in first pregnancies.

Before we proceed farther let us pause for a minute, and simply recapitulate the reasons that we have already given for considering the greater size of the head of the male child as the cause of the greater number of complications and casualties connected with male birth. *First*, I have shown that for the very marked differences existing between the difficulties and perils of male, as compared with female births, there is no other traceable cause, in the mechanism of parturition, except the larger size of the head of the male child. We have seen, *Secondly*, that an increase in the morbid effects may be proved to be connected with an increase in this reputed morbific cause; *Thirdly*, that when the alleged cause is diminished the effects are diminished; and *Fourthly*, that in those morbid complications in labour in which the cause is in abeyance the effect is also absent. *Lastly*, we have found that similar effects upon both mother and child are produced by other causes which are similar in their character and amount.

Now, if in conducting any investigation into the explanation or mode of production of a number of ascertained and acknowledged facts, we found, amidst the assemblage of phenomena submitted to our study, one supposed antecedent condition or probable proximate cause perfectly answering the various reasons and tests to which, in the preceding pages, we have already subjected the increased size of the male over the female infant's head as the alleged cause of the reputed greater danger and mortality attendant upon male births, the evidence, in support of that circumstance as the *vera causa* of these consequences, would be deemed of a very strong and decided character. The evidence would become still more conclusive if it could be shown, that though this *vera causa* appeared at first sight to be in itself an agent quite inadequate for an explanation of the various important effects traced to it, yet, when studied in its full and proper relations, might be shown to have necessarily sufficient power and influence for the production of the results. Such we will now endeavour to prove to be the case with the slight increased volume of the head of the male over the female infant, as the cause of the greater number of the casualties accompanying the birth of male children.

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* "Regarde l'anasarque comme une de leurs causes *essentielles*."—Recueil Periodique de la Société de Médecine, Tom. ix. for 1800–1, p. 110.

SIXTHLY, *The greater size of the male than of the female infant's head is sufficient in itself to explain the greater dangers attendant upon male than female birth, when we consider it in relation to its absolute and cumulative effects.*

"No arguments are required (as Dr Denman observes) to prove that a small body will pass through a small space with more facility than one that is large; the size of the body being supposed to bear any reasonable comparison to the dimensions of the space. Of course it may be presumed that the larger the head of the child is at the time of birth, with the greater difficulty it will be expelled." Nor does the well-known compressibility of the human cranium at birth alter this view in regard to the effects which an increased size of the head is calculated to produce. "For though nature has, with admirable wisdom, (I quote the words of Dr Osborne,) by means of sutures and fontanelles, so constructed the head of the human fœtus that, in the passage through the pelvis it may suffer the form to be altered and the volume to be considerably diminished without such injury to its contents as shall necessarily destroy life; yet as there is a volume beyond which each fœtal head cannot suffer compression with safety—so there is another and still smaller into which it cannot be compressed at all."*

No one will be inclined to doubt the fact, that when the fœtal head is *much* increased beyond the common size at birth, its expulsion will be attended with more than the usual degree of difficulty. But exactly the same effect may be produced by the infantile head exceeding the required dimensions in only the most trifling degree, provided it happens, from the particular conformation of the mother, that the maternal pelvis and fœtal cranium are otherwise very nearly and accurately adapted to one another. In such instances a difference in the size of the head of the smallest amount may involve and change the whole question of the physical possibility or impossibility of the passage of the infant. Dr Hamilton, in his "Practical Observations," when speaking of the occasional difficulty of determining in individual cases in practice, whether the maternal passages are or are not of such dimensions as to allow the child to pass, and whether the infant's head may or may not require to be diminished by craniotomy, strongly remarks, "that in this question even a miscalculation of the *sixteenth part of an inch* might be fatal to the life of the infant." "The author, (he adds in a foot-note,) has been accustomed to illustrate this practical remark to his pupils by a very simple mechanical demonstration. He first shews the smallest possible aperture through which the fœtal head of the ordinary size can be squeezed, and he then covers the head with a common towel, and proves the utter impossibility of its then passing through the same aperture. He does not believe that the addition of a common towel can increase the diameter of the head more than the sixteenth part of an inch."*

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* Essays on the Practice of Midwifery, p. 188.

If Dr Hamilton's calculation regarding the increase of the diameter of the head in this experiment be true, viz. one-sixteenth of an inch, then here we have the transit of the fœtal head entirely prevented by an increase of size which is not more than half of that difference, (one-eighth of an inch,) which we have seen to exist between the diameter of the standard male and the standard female head at birth.

In relation to the question of the greater difficulties attendant upon male births, it ought further to be always held in view, that, when the fœtal head is notably above the medium standard, the infant is, in a great majority of instances, of the male sex. Among the 120 infants whose heads were measured by Dr Clarke only six were above 14½ inches in circumference. These six were all males. Twenty-nine of the children were as high as 8 lbs. and upwards in weight. Of these 29, as many as 19 were of the male and 10 only of the female sex. Such facts require no comment.

But let us throw altogether out of consideration the more extreme cases to which we have adverted, and look upon the question of the more numerous dangers attendant upon male births as the result merely of the average slight increase of the standard male over the standard female head at birth. Even under this limited view the normal difference in size between them (about one-eighth of an inch in diameter), inconsiderable as it may at first appear, will, we believe, afford us a perfectly sufficient explanation of the statistical facts which we have brought forward in the preceding pages, provided we consider what the *accumulated* mechanical effects of this difference in measurement would be when operating over a very wide and extended range of cases of parturition. A few remarks will illustrate our meaning. We take for granted, that the act of parturition consists of the dilatation of the female passages, and of the expulsion of the fœtus through them. Before the process can be terminated, a very considerable amount and continuance of expulsive and dilating force is in general required. During it the parietes of the maternal passages are necessarily subjected to pressure from the advancing fœtal head, and in turn the wedge-like fœtal head itself is compressed by the resisting passages. The whole process is rendered the more tedious and difficult in consequence of the size of the head of the fœtus being nearly proportioned to the size of the passages of the mother. What, then, will be the effects upon this whole process of such a difference in the dimensions of the fœtal head, as holds good with regard to the relative sizes of the male and female at birth, supposing the mother's pelvis and passages to be always, *cæteris paribus*, the same in their capacity? If the female infant's head, when of the standard size, require for its transit a certain extent of dilating and expulsive force,—if it necessarily produce, during its egress through the maternal passages, a certain amount of pressure upon their walls in order to overcome the resistance which they offer,—and if, in turn, the head of the child is, under the same action, compressed itself to a certain degree;—then the standard male infant's head (as being an eighth of an inch greater in diameter, and consequently demanding more actual space for its transit), will require proportionally a greater extent of dilatation in the maternal passages, and a greater expenditure of force to effect this greater dilatation; it will hence also produce, during its egress through the maternal canals, a greater amount of pressure upon their parietes, and be itself reciprocally compressed to a greater degree. The difficulties and dangers to child and mother entailed by this latter state of matters might well *a priori* be supposed to be exactly those that we have shown them really to be, under the various propositions that we have previously laid down.

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* Hamilton's Practical Observations, Second Edition, p. 254.

But that the birth of the male, as compared with the female child, is accompanied with more obstacles and delay, need not be an inference resting solely, as it does above, upon abstract though satisfactory reasoning,—for it is a matter which should admit of direct statistical and arithmetical proof. If our data were sufficiently precise and extensive we ought to be able to prove, that, as an

Eleventh Proposition,

The average duration of labour is longer with male than with female children.

Dr Collins has given a table (p. 22), showing the number of his patients that were delivered in " one quarter of an hour," " in one hour," " in two hours," &c., from the commencement of labour. The sexes, however, of the children are not distinguished, so that the data are of no avail in regard to their bearing upon the proposition we have just stated. Nor am I aware that there has been, as yet, any where published such a series of facts as would enable us to prove statistically that the birth of the male child is absolutelyl onger than that of the female, in the time required for its completion. The average duration of labour in the European females is about four or five hours. The average duration of male births will probably be found, when sufficient evidence is collected, to be about a quarter of an hour or half an hour longer than that of females. I venture to make this allegation, in regard to the common run of labours, upon the evidence afforded by an assemblage of cases, now lying before me, in which the progress was somewhat protracted beyond the usual standard. The cases I allude to were collected under the following circumstances. In 1836 a separate register was begun in the Edinburgh Lying-in Hospital of those labours in which the date of the commencement of the process and the comparative length of each successive stage could be ascertained with sufficient accuracy. Those cases in which the labour was very speedy, and indeed all that were less than three or four hours ill, were not entered, because in most of them the duration of the labour and its stages could not be noted with the accuracy required; many of these patients only entering the hospital a few minutes before delivery. About half of the labours in the Dublin Hospital appear to have been terminated within the first two or three hours,* so that our Edinburgh register, as wanting that proportion of the more rapid cases, cannot show the average length of the process in ordinary instances. The records, however, which it contains of the cases protracted beyond the period mentioned have, I know, been kept with adequate care,† and seem capable of furnishing us with sound and unprejudiced evidence upon the present topic, inasmuch as they were originally noted without any view to such an inquiry as this into the comparative duration of male and female births. In analyzing the register I find, that, from 1836 to 1841 inclusive, 249 male and 178 female births are entered, with notes of the precise length of the labour in each. The following table will show the absolute and average duration of the labours with male and female children in these 427 cases.

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* Of the 15,850 cases noted by Dr Collins, 7063 were terminated within two hours, and 9550 within three hours from their commencement.

† A number of the entries were made by myself when annual pupil at the hospital in 1836-7, and have been continued by others in that office.

Labours.	Absolute duration of the whole cases.		Average duration of each labour.	
	Hours.	*Minutes.*	*Hours.*	*Minutes.*
With male children, [249 in number,]	2646	33	10	38
With female children, [178 in number,]	1702	29	9	34
Average greater length of the male birth,			1	4

In those classes of labour in which the process becomes morbid from tediousness or other complications and difficulties of a more serious character, the difference between the duration of the male and female births would seem to be even greater than the above table displays. At least such an inference appears deducible from the analysis which I have made, from Dr Collins' tables, of the comparative length of the male and female births in the records which he has given of 407 still-born children, (including the putrid and premature), of 97 morbidly tedious labours, of 68 crotchet cases, and of 135 instances in which the mother died in connection with delivery. The preceding numbers include *all* the cases under these several heads in which the exact duration of the labour is mentioned in Dr Collins' tables. The result is this:—

Labours.	The average duration of the male births was,		The average duration of the female births was,		The aver. greater length of the male births was,	
	Hours.	*Minutes.*	*Hours.*	*Minutes.*	*Hours.*	*Minutes.*
With still-births, (310 males and 197 females,)	14	27	12	10	2	17
Morbidly tedious, (56 males and 41 females,)	43	18	40	12	3	6
Requiring crotch., (44 males and 24 females,)	42	22	38	20	4	2
Leading to death of the mother, (86 males and 44 females,	18	9	12	58	5	11

The evidence afforded by the foregoing table appears almost enough to warrant us in qualifying the proposition we have laid down, to this extent, that " the average duration of labour is larger with male than female children, and the difference in this respect between male and female births becomes increased in length when the labours become more severe and dangerous in their character."

The remarks and proofs that we have now offered may tend to remove the difficulty which the inquirer may at first entertain regarding the supposed inadequateness of the slightly increased size of the male head to produce all the greater number of casualties and complications that we have traced to be connected with male births. For it will probably be allowed that the principal, or indeed sole obstacle, which the mind has to contend with, in allowing the very small size of the male over the female infant's head to be the cause of the remarkable differences which we have traced between the results of male and female births, consists in the difficulty of at first supposing so slight an apparent cause to be the agent by which such remarkable consequences are brought about. But in considering this and the analogous questions, it must be held in recollection, that in all processes, whether vital or physical, in which, as in parturition, an established relation of mechanical conditions is required, any disturbance in these conditions, however small and trifling in itself, will ultimately, and when followed out through numerous and extended series of observations, be found to lead to results the magnitude of which could scarcely have been previously surmised. These results may not be appreciable when we confine ourselves to the study of the agency in an individual case, or in a small number of cases merely, but they become more and more marked in proportion as the number of our instances become more and more extensive. The effects cannot be distinctly seen when we look for them in a limited series of data, but they may be evoked with all the force of a mathematical demonstration, when we prosecute our calculations for them among large and accumulated masses of observations. From any study, however minute and accurate, of a limited number of cases of labour, no man would probably feel himself entitled to conclude that male are in any notable degree more difficult and dangerous than female births; but this, as we have seen, becomes a demonstrable and strongly marked fact, when we direct our inquiries after the truth of it into the records of hundreds or thousands of carefully reported observations, such as we have made use of in the course of the preceding inquiry.

The remarks we have hitherto made upon the male sex of the child as a cause of delay, difficulty, and danger during labour, have referred principally to the larger size of the male infant's head or cranium. In that condition, and its effects, we have so far found a satisfactory explanation of the numerous complications and casualties to which the mother herself is subjected in male births. One or two additional observations may be required to show more specifically in what manner the safety and life of the male child during labour, and for some time after it, is endangered by the same circumstance. We believe that the larger size of the contents of the male, than of the female cranium, or, in other words, the larger size of the male brain or encephalon at birth, and the consequent greater compression and injury to which it is subjected during birth, affords us the proper clue to the explanation of these peculiarities in the male infantile mortality.

The weight, and hence the size of the brain, is now well ascertained to be absolutely more in the adult male than in the adult female. From measurements of the interior of numerous crania,* Sir William Hamilton computes the male adult encephalon as weighing 3 lbs. 8 oz., Troy, and that of the adult female as 3 lbs. 4 oz. Professor Reid carefully weighed in the Edinburgh Infirmary the encephalon of 53 adult males and 34 adult females. The average weight of the male brain in these observations was 3 lbs. 2 oz. 3¼ drachms; that of the female, 2 lbs. 12 oz. 8½ drachms; and the average difference in favour of the male encephalon thus amounted to 5 oz. 11 drachms.† This difference in weight between the encephalic contents in the two sexes exists also at birth. In an elaborate paper on the weights of the brain in the different sexes, &c., founded on numerous observations, and published by Professor Tiedemann in the London Philosophical Transactions for 1836, that distinguished anatomist states, (p. 502,) "the female brain weighs on an average 8 ounces less than that of the male, and this difference is (he adds) already perceptible *in a new-born child*." In one of his tables Tiedemann adduces the weight of the brains of two male and two female infants at birth. The average weight of the two male brains was 13 oz. 6 drachms 20 grains; the average weight of the two female, 10 oz. 44 grains; and the average excess of size of the encephalon of the male infant, as deducible from these four observations, 3 oz. 5 drachms 36 grains. But this result is probably much higher than a more extended number of data would give, in consequence of one of the female children in the table being evidently below the standard size and weight of the girl at birth.

There is another way by which, in the present deficiency of direct facts bearing upon this point, we may arrive at the same conclusion with equal or greater certainty. "The weight of the brain," says Tiedemann, "of a new-born child is relatively to the size of the body as one to six." The male infant at birth is, as we have seen, on an average, about nine or ten ounces heavier than the female. If the above ratio between the brain and whole body holds equally true, (as there is every reason to believe), of the two sexes, the male brain should, on an average, be one-sixth part of this heavier than the female brain at the time of delivery. Hence the male encephalon is, in the infant at birth, about 1½ ounces heavier than the female encephalon, and the male brain and cerebellum altogether about one-ninth or one-tenth greater in weight, and consequently in size, than the brain and cerebellum of the female.

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* Monro's Anatomy of the Brain, 1831, p. 12.

† London and Edinburgh Monthly Journal for 1843, p. 322. See also Dr Sim's paper in the London Medico-Chirurgical Transactions, vol. xix., for similar results

The immediate effects, during parturition, of this much greater size of the male encephalon upon its own delicate structures are evident. For as, *cæteris paribus*, the pelvic passages are always the same in their dimensions, the larger and heavier male brain will, in its forcible transit through the given standard space of the maternal canal, of necessity suffer more physical compression and injury than the smaller female head and brain.

Connected with the structural economy of the brain of the new-born infant, there are some circumstances which are calculated to render such degrees of injury, as we have adverted to, more extensive in their influence, and more permanent in their results, than similar amounts of injury of these same parts at other later periods of life. For *First*, the brain of the infant at birth is still so soft and almost semifluid, and its vessels so large, as to be more easily lacerated than years afterwards under the same extent of pressure. *Secondly*, the brain at that particular period of life, and for ten or twelve months afterwards, is undergoing great and rapid normal changes from its imperfect embryonic state to the anatomical consistence, form, and organization which it is to present during the remainder of life. "From birth," observes Billiard, "to the first year, the brain of the infant is in a true state of transition, so that this organ, which is scarcely formed at first, arrives, towards the ninth month or first year, at the organization proper to the brain of adults. Ought we not," he adds, "to attribute to this modification supervening in the brain of infants, the frequency of cerebral affections at the age of which we speak?"*

But there is a *third* peculiarity, probably even more important than the two others. The human brain is very much larger in proportion to the body in the infant, than it is in the adult. In a full-grown adult the proportion of the brain to the body is as about 1 to 40; in the infant it is, as we have already stated, about 1 to 6, or nearly seven times greater.

The influence of this immense difference is important both physiologically and pathologically.† The nervous system in the infant is thus rendered naturally much more susceptible and sensitive of physical and sympathetic morbid impressions than at a more advanced age;[*]—pathological irritations and disturbances of other parts are far more readily and strongly reflected upon the nervous centres;—and, for the same reason, any direct lesion or disease of the brain itself comes also in turn to exert a more potent and extended effect upon the general and individual functions of the body, than at other later periods of life. All these conditions are heightened and increased in the male, as compared with the female infant at birth, both, *first*, by the greater absolute size of the male brain; and, *secondly*, by the greater lesions and injuries to which it is subjected during the process of labour. I have collected some statistical data to show that the particular diseases which destroy so many more male than female children immediately after birth and for a short time subsequent to it, and which are therefore the evident and demonstrable causes of the excess of the male over the female mortality at that early period of life, are all such as are capable of receiving an explanation of their more marked frequency, severity, and danger upon the above principles, in conjunction with the consideration of the increased size and increased injuries, at birth, of the male as compared with the female encephalon. To adduce evidence of this to the amount that might be necessary, would require us to extend this essay far beyond the limits prescribed to it, and these it has already far overstepped. I may, on another occasion, return to the subject; and, in the meantime, shall only add, that the particular affections alluded to, as producing that excess of the male over the female mortality, which is so remarkable at birth, and progressively decreases from that time onwards, are almost all referrible in their origin or course to morbid states and disturbances of the nervous

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* Billiard, Traité des Maladies des Enfans, p. 335, Brussels' Ed.

† Tiedemann, on whose authority I have given the respective proportion, stated in the text, of the whole brain in the infant and in the adult, observes, in relation to

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this point, (and his words apply equally to the morbid as to the healthy states of the system), " The different degrees of susceptibility and sensibility of the nervous system seems to depend on the relative size of the brain as compared with that of the body. Children and young people are more susceptible, irritable, and sensible than adults, and have a relatively larger brain. In diseases which affect the nourishment of the body, the susceptibility increases as the patients grow thinner. The susceptibility and sensibility decreases, on the other hand, with persons recovering from a long illness gradually as they regain their strength. The degree of sensibility in animals is also in proportion to the size of the brain. Mammalia and birds have a large brain, and are more susceptible than amphibious animals and fishes."—London Philosophical Transactions, Vol. cxxvi., p. 503.

[*] " The nervous system in infants is naturally in a very susceptible condition; it is consequently easily excited on the application of the slightest impressions. This constitutional irritability is characteristic of the infant state. . . . . The nervous sensibility is in excess at this age, and whatever be the disease with which the infant is affected, a morbid excitability and irregularity of action in the nervous system are more or less its attendants."—Stewart's Practical Treatise on the Diseases of Children, New York, 1841, p. 494. See also Maunsell and Evanson on the Diseases of Children, p. 13, &c.

system,[*] and to functional derangements and inflammatory action in it and other organs of the body.

CHAPTER IV.—PRACTICAL INFERENCES, AND EXTENT OF INFLUENCE EXERTED BY THE MALE SEX OF THE INFANT UPON THE GENERAL MATERNAL AND INFANTILE MORTALITY DURING PARTURITION AND FOR SOME TIME SUBSEQUENT TO IT.

The subject of the preceding inquiry is certainly not without interest in a physiological point of view. Nor is it devoid of importance in a pathological and practical light. The results of the whole investigation show us with demonstrative force that the adaptation of the fœtus to the maternal passages in human parturition is, in general, so close and perfect, that a deviation in their relative size of the slightest possible extent, is capable of altering immensely, when the subject is viewed on a large scale, the consequences of the process as regards both the immediate safety and life of the mother and infant, and their subsequent welfare. We may be hence led to see more strongly than previously the advantages attendant upon the presentation of the head of the infant in its most natural position, and hence in its smallest given diameter, and the disadvantages accompanying any deviations in its presentation, seeing that all these deviations offer a larger circumference than the normal parietal case. The same inquiry illustrates well the importance of saving, and particularly in cases in any degree tedious, or in any slight malpresentation, the space of the pelvic passages as much as possible, by keeping the rectum and bladder empty, by carefully guarding against congestion and tumefaction of the soft tissues lining the pelvis, and by promoting, by all appropriate means, the dilatability of the maternal passages. We can easily, from the same data, understand how greatly the dangers of the mother and child are increased by organic contractions, however slight, in the pelvic passages and bones themselves, and by increased volume on the part of the infant, whether that increased volume be the effect merely of excessive growth or the result of actual disease.

The object of our investigation becomes one of still more practical moment when we consider it in relation to the extensive character of its operation and influence.

The importance of any cause of human disease does not so much depend upon the immediate intensity and danger of the morbid action which it is calculated to excite in single individuals, as upon the frequency and extent of its operation, and the consequent amount of its effects upon the general community. In this way a slight disease or slight cause of disease, which acts upon a great proportion of the population, may become as important in its ultimate and practical results as a malady of the most formidable character, provided that malady is much more narrowed and limited in its attacks. "The cholera," observes Quetelet,* "and the influenza are diseases which differ greatly from each other; the one is a dreadful scourge, which manifests itself in the most dreadful manner; the other, in its ordinary external appearance, resembles a catarrh or common cold; and yet the tables of mortality prove, that, although the latter disease is not so deadly, it nevertheless, in consequence of its universality, and in consequence of the sufferings it causes, produces results nearly as extensively fatal as cholera. Facts serving to confirm this opinion may be found in the excellent work published by Dr Gluge on the History of Influenza."†

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[*] "The diseases of the nervous system are *twenty-three per cent.* more fatal to males than females, *the chief difference arising from the diseases which affect children.*"—Mr Farre in the Registrar-General's Second Annual Report, Appendix, p. 4.

The remark which we have applied to diseases and the causes of disease holds equally true with regard to morbid complications in midwifery and their causes. Those special states which, in obstetric practice, are seen to lead to some of the most formidable causes of difficult labour, such as tumours in the pelvis, malpositions of the infant, inertia of the uterus, &c. are certainly often most disastrous in their immediate effects, but yet, upon the whole, they are comparatively so rare in their occurrence as not individually to lead, upon a large scale, to consequences of so severe and fatal a character, as disturbing agencies of a far slighter, but far more general kind, such as that inconsiderable enlargement of the male over the female head, which we have been considering in the preceding pages. A few computations will best illustrate and enforce the truth of this abstract remark. These computations I shall found on the idea that the bases of calculation offered by the data of the Dublin Hospital are sufficiently correct to serve for a ground of analysis of other analogous statistical results. To avoid as far as possible any probability of great errors, we shall keep our calculations considerably within the limits that our apparent standards might seem to warrant.

With this explanation, let us proceed to inquire to what extent the cause of difficulty and danger during labour, which forms the topic of our present essay, fatally influences each single year in England, or during a succession of years, the results of parturition, as respects the fate both of the mother and infant. Some facts, recorded by Mr Farre, in his admirable contributions to the different annual reports of the Registrar-General, will serve as matters of comparison on these points with the Dublin Hospital returns.

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* See Natural History of Man, p. 112.

† Die Influenza oder Grippe, u. s. w. Minden, 1837, 8vo.

Number of maternal deaths referrible annually in Great Britain, to the influence of the comparatively great size of the head of the male infant.

In the returns of Drs Clarke and Collins, we have reports in the Dublin Hospital of the sex of the child in 368 cases in which the mother died from labour or its consequences. In 231 instances the child was male; in 137 cases it was of the female sex. The proportion of maternal deaths, after male and female births, was therefore as follows:—

Total deaths	With male children.	With female children.	Proportion of males to females.
368	231	137	168 to 100

If we venture, then, to compute from these data, that out of 250 mothers that die in child-birth from parturition or its effects, 150 have given birth to males and 100 to females, as the above table would seem to show, then we have in every 250 maternal deaths an *actual excess* of 50 cases of loss of the mothers after male births, for which, (for reasons which we have previously stated at length,) we can find no other explanation than the greater comparative size of the male head, and we have already attempted to prove that this explanation is in itself logically and amply sufficient to account for the consequences which we have attributed to it. In other words, among every 100 parturient mothers that die, we have 40 perishing after producing female children, other 40 perishing after producing male children, and the remaining or additional 20 in the 100 perishing also after the birth of males, but so far forming a regular and constant *excess* of 20 per cent. of deaths in connection with male births, traceable to no other cause than the sex and consequent size of the infant. One in every 5 maternal deaths, or 20 in every 100, and 200 in every 1000, are so far the direct or indirect consequences of the greater dimensions of the head of the male infant. Now, at the present day in Great Britain, upwards of 3000 women die every year from child-birth or its immediate effects. Hence, according to the above computation, 600 of these 3000 cases,* as forming the *excess* of male over female births, are more or less immediately the results of the sex and size of the male infant. In Great Britain, therefore, the valuable lives of 500 mothers, (to speak within the terms,) are lost in childbirth through the influence and agency of the cause in question.

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* During the four years, from 1838 to 1841, there died in childbirth in England and Wales 11,722 mothers. In 1841, the last year of which the returns are yet published, 3007 women died in childbed in England and Wales, (Fifth Annual Report of the Registrar-General, p. 380.) If we add to these 500 deaths as occurring in childbed in Scotland, we will be much within the limits in computing 3000 maternal deaths to take place annually in Great Britain from labour or its immediate consequences. I reckon 500 deaths as occurring annually in Scotland in childbed on the calculation which seems to hold in regard to England, that nearly 200 deaths happen every year from this cause in every million of the general population.

Number of infantile deaths occurring annually in Great Britain during labour, and referrible to the sex and size of the male infant.

We have seen, under our fourth proposition, that in the Dublin Hospital there died, during the process of parturition, and probably as a consequence of the injuries to which they were subjected, 151 male children for every 100 females. According to the mode of argument followed in the preceding paragraph, there was thus an *excess* of 50 male deaths among every 250 children, or 20 in every 100, referrible to the greater size of the head of the male infant. Further, we may take it for granted that, on a low computation, 1 in every 50 children, born dies during labour, about 1 in every 25 cases being a still-birth. To be certain, however, not to overstep our limits, let us reckon only 1 in every 75 children to die during parturition, and 1 in every 5, or 20 per cent. of those that thus perish to be formed by that excess of the mortality of males over females which we can trace to no other cause than the influence of the greater dimensions of the male head. In England and Wales about 500,000 births take place annually.* By the above computation more than 6500 of the offspring of these births die during labour, and one-fifth of that number are lost in consequence of the sex and size of the male child. In Great Britain, therefore, the lives of 1500 infants are annually lost in childbirth from the operation of this agency.

Number of infantile deaths occurring annually in Great Britain within the first year of life, referrible to the influence of the sex and size of the male child during labour.

In 1841 there died in England and Wales, within the first year of life, 41,444 boys and 32,766 girls. There was thus an excess of 8678 deaths of male infants within the first year, and 3610 of this excess occurred within the first month after birth. In considering this subject under our seventh proposition, I have already stated our reasons for considering the excess in question of male over female deaths, in very early infantile life, as owing to the greater injury sustained by the head and nervous system of the male child in its passage through the pelvis during delivery. I have further commented on the same subject in speaking of the excess of size of the male head and encephalon as the probable cause of the greater difficulties attendant upon male births. Under these points enough has been stated to show that a large proportion, if not the whole, of the remarkable excess of the male over the female mortality, which is observable for some time after birth, is traceable to the greater dimensions of the male head and encephalon, and their consequent greater compression and injury during delivery. Supposing these views to be perfectly correct, there die annually in Great Britain upwards of 5000 children within the first year after birth, whose death is referrible to the influence of the sex and greater size of the male head during labour.

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* "In 1841, 512,158 births were registered, in 1840, 502,203, and in 1839, 492,574." Registrar-General's Fifth Report, p. 8. It seems doubtful if all the births that occurred were registered, as this, like the other branches of registration, could not at once be made perfectly efficient. Certainly it is to be greatly lamented that there is no official return of the still-births. The omission is a most extraordinary one, and surely ought to be corrected.

If we add together the three series of observations that have been stated under the preceding heads, the result is as follows: *upwards of 7000 deaths in all, namely, above 6500 of the deaths of infants during and after birth, and 500 of the deaths of mothers in childbed occurring annually in Great Britain, are referrible to the direct or indirect agency of the cause that we have been discussing, viz. the sex and larger size of the head of the male child.*

In using here the term *cause* in reference to the slightly greater size of the male head as the source of the results that we have above ascribed to its influence, it may be proper to state the precise meaning which we attach to this term. Amidst all the uncertainties of medical language, there is no phrase which has had more vague and contradictory significations attached to it. Except in the cases in which disease and death are produced by the immediate action upon the body of severe physical injuries, or strong chemical agents, it rarely or never happens that serious and fatal morbid actions are excited in the economy through the operation of one single morbific cause. In most, if not in all, other instances, a number of causes are found to have acted either in concourse or succession towards the production of the disease that may be present, and the effect to which it leads. According to the order which these several causes may occupy in the series or chain of antecedent sequences, and according, in some instances, to their supposed relative importance and intensity in the production of the existing morbid action, they are individually described by pathologists as accessory or determining,—remote or immediate,—predisposing or exciting,—in relation to the disease that has been originated by their combined influence. Hence a cause which may be termed predisposing in one case may be more properly designated exciting in another, or the reverse, and consequently different authors often describe the same cause under opposite heads.* Further, it seldom occurs during the course of a fatal disease that the same cause which had especially excited the original malady continues, by the intensity of its own immediate action, to lead to the fatal result. An example will illustrate our meaning. All pathologists look upon excess in eating and drinking as a common cause of disease. If the body be debilitated by previous morbific agencies, a fit of intemperance may suddenly excite in it some serious and deadly morbid action, as extensive effusions or inflammation, severe fevers, &c., and thus act as an *exciting* cause of these diseases; or when the powers of the constitution happen at any time to be impaired by intemperance, the application of an excess of cold or heat may lead to similar consequences, and then the cause we are considering, (the intemperance), would be regarded as a *predisposing* one. Further, it rarely happens that intemperance in eating or drinking leads to death by the mere excess of their own action and influence on the body. Few men, in other words, die of direct gluttony, or direct intoxication; but many die from intemperance in eating and drinking, leading directly or indirectly to a variety of maladies, such, according to Dr Copland, as "plethora, inflammatory complaints, functional and organic diseases of the stomach, liver, and bowels, gout, apoplexy, paralysis," &c.†

The view which we have stated as generally taken by pathologists with regard to intemperance as a cause of disease and death, applies exactly to the greater size of the male head as a cause of the greater number of complications and casualties connected with male births. In some instances this excess of size acts as a predisposing cause to the accidents and diseases that are consequent to it, and, without its antecedent or concurrent agency, these accidents and diseases would not be produced. In other instances, it acts as a determining or exciting cause of the morbid conditions and complications resulting from its operations. We will take an illustrative example from an accident which we have already spoken of in a previous page, viz. rupture of the uterus. When rupture of the uterus occurs in a patient, who has already borne a large family, and where the uterine parietes are, from some preexisting disease, much more weak and lacerable at one part than another, the greater delay and difficulty of the labour in connection with the larger sized male head may give rise ultimately to the complication in question, when the shorter and more easy passage of a smaller fœtal head would not have led to the same disastrous consequence. Here the previously diseased state of the uterus acts as the predisposing, whilst the larger size of the male head acts as the exciting or determining cause of the lesion. On the other hand, when rupture of the uterus occurs in a first labour in consequence of the larger size of the male head calling forth a morbid and inordinate degree of uterine contraction for its expulsion, we have this inordinate uterine contraction acting as the exciting or determining cause of the injury, and the excess of size of the male head standing now in the relation of the predisposing cause to it. Further, like other pathological causes, the excess of size of the male head does not so often lead to accidents and death by the mere influence of its own intensity, or by the effects of direct compression upon the maternal passages or infantile head, as by leading to the supervention and excitement of various other formidable and fatal complications, such as morbid delays in the labour demanding instrumental interference, eclampsia, post-partum hæmorrhage, puerperal fever, &c. on the part of the mother,—and convulsions, nervous and inflammatory diseases, &c. on the part of the infant.

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* For instance, Drs Copland and Craigie, two of the latest British writers on practical medicine, speak of intemperance in food and drink under two different heads. "Amongst the most frequent *predisposing* causes to disease is," says Dr Copland, "intemperance in food and drink." (Dictionary of Practical Medicine, Part II. p. 562.) Dr Craigie enumerates the same morbific agency among his exciting or occasional causes of disease. (Elements of the Practice of Physic, p. 18.) Dr Williams, again, describes excess of aliment among his exciting causes of disease, and habitual excess in stimulating drinks among his class of predisposing causes. (Principles of Medicine, 1843, pp. 9 and 24.)

† Medical Dictionary, Part ii. p. 562.

Under the signification and reservations which we have thus stated, we repeat that the greater size of the male fœtal head is the more or less immediate *cause* of a large number of those maternal and infantile deaths that take place in connection with labour, or as a result of that process. In illustration of this remark, I shall venture to add another observation,—one which may at first seem sufficiently startling to those who have not practically directed their attention to the subject, though it is but a simple and direct deduction from the facts we have so imperfectly brought forward in the preceding pages. It is this:—

The official returns of the mortality of England and Wales have only, as yet, been collected for somewhat upwards of seven years, viz. from 1st July 1837 to the present date. If the calculations we have already given are accordant with truth (and we believe them to be much within the limits), *there have been lost in Great Britain during that limited period, as a consequence of the slightly larger size of the male than of the fœtal head at birth,* ABOUT 50,000 LIVES, INCLUDING THOSE OF ABOUT 46,000 OR 47,000 INFANTS, AND OF BETWEEN 3000 AND 4000 MOTHERS WHO DIED IN CHILDBED.

ART. VII.—*Contributions to Midwifery.* By WM. SMITH, M.D. Edinburgh, Physician to His Royal Highness the Duke of Cambridge; Licentiate of the Royal College of Physicians, London; Member of the Royal College of Surgeons, &c. &c.

ON THE POSITION OF THE PARTURIENT WOMAN.

ALTHOUGH the position of women during labour has been a subject of important discussion, it nevertheless still varies considerably according to the custom or habits of different countries. In some the back is preferred, in other places the side, and in not a few districts the sitting and even the kneeling postures are adopted.

Before we can understand the best position in which to place a woman in labour, it is indispensable that we know the axes of the pelvis, especially that of the inlet,* with which we have more particularly to do in the following inquiry. This embraces a knowledge of the inclination of the pelvis, which is marked by the angle that an imaginary line, indicating the axis of the superior aperture, forms with the horizon when the woman is in the erect posture. But as these are found to differ in different individuals, it becomes necessary to fix upon a standard, by a comparison with which the extent of various deviations as to size, form, &c. can only be ascertained with any degree of accuracy.

The standard pelvis, then, is one of sufficient capacity to easily permit the passage of a full grown living fœtus, and its inclination such (according to Burns) as to form with the horizon an angle of 35 to 40 degrees when the body is erect. Or, according to Smellie, its brim is horizontal when a woman is "half-sitting half-lying," and an imaginary straight line then extended from the umbilicus to the coccyx would pass through the centre of the brim, which line forms with the horizon an angle of 50 or 60 degrees.

Most authors agree in the above description of the axis of the brim, as well as the inclination of the pelvis; yet there is still much difference of opinion regarding the position of the woman during labour, each having arguments in favour of the position he prefers—believes it more compatible with the safety of the woman and his own convenience. Again, the favourite position of an accoucheur or country practitioner is in general exclusively adopted, without any regard to the conformation of the individual in labour, although it is agreed on all hands that many great anomalies exist.

I maintain, therefore, that to adopt implicitly any one or other of these exclusively, is decidedly incorrect; because, as all women are not formed alike, so it must happen, that, just in proportion as the difference exists, must their position vary during labour. In one individual the position on the side may be the best, while in another that upon the back should have the preference; or there may be such a conformation of pelvis or body present as to wholly forbid the position on the left side, while that upon the right would effect easy and speedy delivery.

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* In all cases where I speak of the "axis of the pelvis," I refer to the inlet, unless otherwise expressed.

This may appear a startling hypothesis; yet I shall, I hope, clearly demonstrate its truth in the following pages.

While I admit the necessity of being thoroughly acquainted with the various dimensions of the female pelvis, and the no less importance of accurately understanding its axes and inclination in reference to practical midwifery, I at the same time assert, that, as these latter are neither the same in every individual, nor identical, even in a single individual, in every position of the body, it is as important to properly comprehend the effects of various positions, in changing the axis and inclination of the pelvis, as it is to be accurately acquainted with these in any single position of the body; because the inclination of the pelvis, and consequently the axis of the brim in reference to the uterus, may be materially altered by the position of the woman during labour. Therefore, ignorance of the effects of position must, under varied circumstances, bring discredit upon the practitioner and prolong the sufferings of the woman, so far as it may, in any case, be a cause of protracting the progress of labour.

Whatever diverts the axis of the pelvis from that of the uterus protracts,—whatever adapts the one to the other expedites delivery. In all cases, therefore, of malformation or deviation from the standard, the greatest desideratum is attention to the woman's position, taking it for granted that there is sufficient capacity of pelvis to permit the free passage of the child. Nay, in well-formed women the actual process of parturition may, and doubtless has been, sometimes protracted by their position during labour.

Having premised these remarks, I shall consider,

1. The variations that may exist in the inclination of the pelvis.

2. The means of altering the inclination of the pelvis, and hence altering its axis so as to adapt it to the axis of the uterus; and, as I proceed, I shall offer some remarks on the benefits arising from such alterations in certain cases, as well as the injury that may accrue to others.

1. Supposing a person in the erect posture, the inclination of the pelvis may vary from what is considered the standard in three particulars, and yet be of standard capacity.

a.—Its brim may approach the horizontal.

b.—Its plane* may be nearly perpendicular to the horizon.

c.—It may be what I shall here term a "hitched pelvis," and at the same time may partake more or less of either of the former conditions.

In the first of these the angle which the axis of the brim forms with the horizon is nearly as obtuse in the erect position as that described by Smellie when the woman is half-lying half-sitting. In the second the angle thus formed is much more acute; in the one the plane of the pelvis looks more directly upwards; in the other, it looks more directly forwards than the standard inclination.

2. The power we possess of altering the position of the pelvis, which is, in other words, altering its inclination, will be best understood by an individual placing himself in different attitudes. He will then find, that whatever change the position of the pelvis undergoes in reference to the trunk will take place when he bends the spinal column forwards, backwards, and sideways; and, moreover, that this change, by which actual parturition may be expedited or retarded, under existing circumstances, is effected only through the elasticity of the spine so far as it is composed of the last four or five dorsal and first three or four lumbar vertebræ.

Now, suppose one called to attend a woman in labour in whom (a) the first described position of pelvis exists, let us think for a moment what would be the effect of allowing her to bend forwards, as most authors assert is necessary in their descriptions of the position women should assume in actual parturition. Would it not be to cause the uterus to form an angle with the axis of the inlet of the pelvis in a greater or less degree, according to the extent to which the woman bends forwards? In such a case bending forwards is absolutely injurious to the progress of labour. Again, in cases when (b) the second described condition is manifest, we shall as clearly see the necessity of bending the body forwards upon the pelvis as of guarding the patient against doing so in the former case. Regarding (c) the third described position, or conformation of pelvis, I shall speak afterwards.

In order that these two conformations of body may not be called more imaginary than positively existing, I beg to refer to what Dr Rigby says under the head "Inclination of the Pelvis." "The angle, (says this excellent author,) which the axis of the superior aperture forms with the horizon, when a woman is in the erect posture, necessarily marks what has been called the inclination of the pelvis, and varies of course in proportion to the angle which the above axis forms. In a tall woman, of slender figure, where the different curves of the spinal column are slight, the inclination of the pelvis is much less than in a short thick-set woman, where the spine is much more strongly curved. Where the inclination is slight the hollow of the sacrum is generally small, and the vulva directed more forwards; where, on the other hand, the pelvis is much inclined, the hollow of the sacrum is observed to be deep, and the vulva directed more or less backwards.

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* I use plane and brim synonymously.

Again, Dr F. H. Ramsbotham says, "By a knowledge of the axis of the trunk and pelvic entrance we can place our patient in the position most favourable to the easy descent of the fœtal head through the brim into the cavity; this is on the side, (the left is usually preferred in this country,) with the shoulders thrown forwards, the back bent into a curve, the thighs drawn up towards the abdomen, and the legs flexed towards the thighs. In this position the two axes are brought more nearly into one line than in any other, and the head is directed more completely over the brim.

From those two quotations, which I believe represent pretty nearly the sentiments of most British authors, as well as those of other countries, two very important questions arise; 1. Is the position just described as suitable to a tall woman, the inclination of whose pelvis is much less than that of a short thick-set woman, as it is to the latter? 2. Can the position on the left side be made applicable to all cases?

In discussing these points, it must be remembered that the question is not regarding the axis of the trunk and pelvis, but in reference to that of the uterus and pelvis.

In some cases I have already said it may not only be beneficial but absolutely necessary to bend forwards as above mentioned; yet in others, as I have shown, it is most injurious to do so, and may be one grand cause of protracting labour; because, if the back be bent into a curve, the fundus of the uterus is necessarily forced forwards; while, at the same time, the inclination of the pelvis is very much changed; and though its brim may thus be brought into the axis of the trunk, it is so much the more thrown out of the axis of the uterus, which is never in the axis of the trunk. The propelling powers, therefore, of the uterus will be directed towards the promontory of the sacrum, provided the conformation be that I have first described, viz. that in which the plane of the pelvis approaches nearly the horizontal.

A case illustrative of the above facts occurred to me when a pupil at Minto House, Edinburgh.

The woman to whom I allude was the mother of several children. When I visited her I found the *os uteri* fully dilated, but the child's head had not entered the pelvis. I momentarily expected its descent, and considering it a labour that would soon terminate, and a good case to ascertain the mechanism of parturition as described by

Naegele, I kept my fingers in contact with the vertex; but growing weary of my position without finding any advance of the child, I began to inquire into the nature and causes of delay. I found, in the absence of a pain, I could push the presentation up a little, and in the presence of one I could pass my fingers between the *symphysis pubis* and right parietal bone, which latter gradually became the most depending part, so much so that I could at length feel the ear above and behind the *symphysis pubis;* this also, as the pains were powerful, became much more easily reached, so that I could, after some time, reach the mastoid process of the temporal bone. I feared the head was about to become impacted; but this apprehension was removed by its slight mobility as the pain went off, as well as the capacity of the pelvis, which was very large. I could discover no cause for the delay and suffering, which appeared more than ordinary under all circumstances. I was called cruel if I did not use some means to relieve her. I withrew my hand and desired that she might place herself higher in the bed, or, in other words, stretch herself from the curve which she formed in the centre of it, and not to exhaust herself by any efforts of her own, but allow the pains to act of themselves. She obeyed my directions, and to my utter astonishment, by the very next pain the head must have descended into the pelvis; for, upon its recurrence, she in a smothering tone called me to her assistance. I was not in a hurry towards her, as I could not believe, from my experience of the case, that any material change could have so soon taken place; but as the pain doubled on itself, she cried out, "Will you let me die without helping me." This caused me to make an examination instanter, when I found the head as far as the vulva, which I by no means expected; parturition, however, was immediately completed.

The above case made such an impression on my mind regarding the position of the woman, that I have since then made some inquiries into its nature; for let it be understood, I did not alter the woman's position in the case just related from any previous knowledge of any benefit likely to arise from such practice farther than to give momentary relief and guard against exhaustion; the result I have now faithfully detailed I never for a moment anticipated.

Another case occurred to me shortly afterwards, which impressed me with a still stronger idea that the position of the woman had much to do, *cæteris paribus*, in her speedy delivery. When I visited this latter patient, (who was also the mother of several children,) and had made an examination, I found the *os uteri* considerably dilated and the membranes entire. As the head had not descended into the pelvis, I concluded a considerable time must elapse before the termination of the process of labour, yet nothing could then justify my leaving; accordingly, I sat down to talk to the patient's mother; we had not, however, conversed many minutes, when my ear met the almost breathless cry of "Come fast, fast, Doctor."

I thought there was more than common in the tone of the request, and accordingly lost no time in making an examination, when I was no less astonished than frightened to find that the head, with the membranes entire, had actually passed the vulva; nor was it prematurely small, but the pelvis was certainly very capacious. I scarcely had time to rupture the membranes before the body was expelled, so that I was afraid the child would be drowned, if I may use the expression, in its own *liquor amnii*. On the air being admitted, however, it respired, and did well afterwards. As in the other case, I saw nothing at the time particularly faulty in the woman's position, so in this one I could observe nothing beneficial; yet I may remark she lay neither on her side nor on her back, but observed a sort of medium position, the utility of which I shall speak of afterwards. The former, however, I consider, illustrates very clearly the injurious effects of too much curving of the body, which is but too common in extreme suffering, and ought, in the majority of cases, to be guarded against.

In no case is the bending of the body forwards upon the pelvis more applicable than in that form of pelvis whose plane looks almost directly forwards when the woman is erect; where, if a line were let fall in the axis of the trunk, instead of its falling on the *symphysis pubis*, (as Dr Ramsbotham mentions it would do in a well-formed woman,) it might actually fall one or two inches in front of the pubis. I have seen two skeletons, one in the class-room of M. Breschet in the Ecole de Médecine in Paris, and another in Dr Churchill's class-room, Dublin, in either of which, if a line were let fall immediately in front of the lumbar vertebræ so as to touch them, it would not pass into the pelvis. The latter gentleman, however, told me he thought something might be attributable to the manner in which the lumbar vertebræ were articulated, "but certainly not so much as to warrant us in saying its original inclination was anything like what is considered the standard." In such a case an imaginary line extending from the umbilicus to the coccyx, instead of passing through the centre of the brim, would come near to, if not touch upon, the promontory of the sacrum, against which, one might imagine, the child's head would be directed or forced during labour, but such is not the fact; this it readily escapes and rests upon the pubic bones, where it might for hours remain if the woman did not bend very much forwards, by which the plane of the pelvis, as I have already shown, may be altered and brought into the axis of the uterus. Here, then, are two very opposite positions to be adapted in two conformations of body, when, to all external appearance, nothing perhaps could be detected abnormal; in the one, when the plane of the pelvis looks directly forwards, the woman can scarcely bend too much forwards; in the other, when the plane approaches the horizontal, the woman must keep the trunk quite straight, nay, even bend backwards, by which means alone the axis of the brim may be adapted to that of the uterus. Of course I speak now of extreme cases; but it will be observed that the first case I have detailed might be considered one to which the term "standard" might be applied, yet by the curving of the spine labour was protracted; hence, if it be necessary to pay attention to the position of the parturient woman who is well formed, it is certainly much more so in a mother whose pelvis, spine, or body is found abnormal.

I come now to speak of the third form of pelvis, viz. that which I have designated a hitched pelvis, yet sufficiently capacious to permit a full-grown fœtus to pass through it.

It may not be improper to explain shortly how an originally well-formed pelvis may become hitched, *i. e.* one side rising higher than the other, which not unfrequently happens in neglected *morbus coxarius.* Mr Hunter accounts for the great lengthening of the limb, in a certain stage of this disease, by the oblique position in which the patient places her body in standing upon the sound leg, when the diseased head of the femur and glenoid cavity are unable to sustain any pressure; consequently, Mr Hunter found the tuberosity of the ischium on the diseased side situated much lower than that of the other.

Lateral curvature of the spine, also consequent upon diseased action, is another cause of a hitched pelvis; and if it be considered in reference to the lumbar and last three or four dorsal vertebræ, (the usual seat of the lower curve,) the pelvis is indeed very much hitched, but not in reference to the mesian line of the body. For, according to Mr Stafford, "if a curve takes place in the lumbar region thus, (there will be in the dorsal regions a consecutive curve inclining to the opposite side thus,) generally occupying the upper part of the dorsal and nearly the whole of the cervical region; all this is but an effort of nature to maintain the upright position of the body, the head being thus supported opposite the centre of gravity."

This coincides with Sir C. Bell's views on this subject. He says the spinal column forms a double curve like the "italic *S*," and is at the same time often twisted. "After (says Sir C.,) the distortion fixes in a moderate degree, being observed only in the manner in which she thrusts her shoulder forward and in the prominence of the hip."

Dr F. H. Ramsbotham, in speaking of deformity of the spine, says, at p. 50, "it by no means necessarily follows that the pelvis is distorted. By the annexed cut it will be seen that, although the spinal column has suffered lateral curvature to an extreme degree, yet the pelvis possessed the ordinary diameter at the brim, and the outlet is so slightly diminished in its proportions, that the fœtus will be expelled through it with great facility. Instances of this kind are to be met with in every museum."

Considering such a configuration of body in reference to the obstetric art, we must naturally ask ourselves two questions; 1*st*, Is the mesian line of the uterus in the mesian line of the pelvis? 2*d*, Has the position of the woman during labour any influence upon either?

To the former of these questions we may at first sight be inclined to answer in the affirmative, because the centre of gravity is maintained; but we must remember that, although the head is kept in the centre of gravity, yet the curve that insures this takes place almost completely above the fundus of the uterus; "generally, says Mr Stafford, occupying the upper portion of the dorsal, and nearly the whole of the cervical regions." So that the *fundus uteri* cannot be higher than where the first curve ends, and the second begins; and its body, in fact, following the direction of the lower curve, makes its axis very oblique in relation to the pelvic brim.

Again, we find that curvature of the spine may take place, according to Sir C. Bell, "even when not affected by disease properly speaking, but distorted by mere debility and indolence."

"A young person (says he) lounging indolently, stands naturally on the left foot, throwing the right into a position of relaxation. The effect of this is to incline the pelvis, on which, as on a basis, the spine rests. The spine is projected to the left side at its lower part, and, to preserve the balance, the body is twisted to the opposite side above."

The views of Mr Stafford on this point also coincide with those of Sir C. Bell.

"The immediate causes," says Mr Stafford, "of lateral curvature arise from bad habits, such as standing on one leg and resting the other, from improper use of the muscles of one shoulder, from leaning forward too much on one side, and in a twisted position in writing," &c. page 223.

Now if it be a fact, that a person lounging indolently stands naturally on the left foot, and also that a twisted position in writing, as well as "lying in too soft a bed with a high pillow," (Stafford,) are frequent causes of curvature of the spine, (and I doubt not for a moment the accuracy of these observations), it is a most fortunate circumstance that the most frequent operating causes should be those to elevate the pelvis on the left side instead of the right, when we take into consideration the all but imperative position on the left side, especially in this country, which women assume in labour.

To explain how the above causes operate in hitching the pelvis to the left side is scarcely necessary, for any one by standing on one foot will discover that the pelvis is raised on that side, and lowered on the other, which is relaxed. The left foot, therefore, after supporting the body thus for a time, twists the pelvis to the left side; and if the reader will place himself before a table in the attitude of writing, he will find the very same effect is produced upon the pelvis, viz. the crest and ribs on the left side made to approximate each other; which also happens, as already quoted, by resting on a "soft bed," &c. because the right side is that most usual, not only for children but adults, to rest and sleep upon. If, therefore, it can be said to be a cause of curvature of the spine, it must be one to produce the same effects as those of writing or standing on the left foot. In such cases the left side in parturition is that most suitable; the effect of which will be to separate the crest of the ilium and false ribs on the same side, whilst those of the opposite side (already too much separated) are made to approximate, the inferior curve is thus diminished for the time, and the centre of the brim thereby brought more into the axis of the uterus.

This brings me to answer the second question, page 9, viz. Can the position on the left side be made applicable to all cases? To this I answer, that it cannot; for supposing that either, or any of the above causes, might produce curvature of the spine in an opposite direction, then I say that the position on the left side in such cases would be a very improper one, tending to increase rather than diminish the spinal curve, and consequently to hitch the pelvis still more.

Let us suppose an individual called upon to attend a woman in labour, who, in early life, had had the left limb amputated, and in whom, by the weight of the body being constantly thrown upon the right, the pelvis had become elevated on the right side, and lowered on the left. If he be a *tout à fait* left side man he places his patient in this position, if she has not already assumed it, and then makes an examination; he may probably find the *os uteri* considerably dilated, but the head still above the brain; he knows that some time must elapse before labour is terminated; yet he fears to leave, from the prognosis he forms upon the dilatation or dilatability of the soft parts. Thus he may spend a dreary night with his patient, and in the morning find little if any advance, (which is by no means uncommon even in well-formed women), and judging from what is past, he may then conclude that he will have time to make his usual round of visits before his services can actually be required. This idea, forced still stronger on his mind by the anxious sufferer constantly looking to him for relief or assistance, acts powerfully in causing him to make his exit; remembering the fact, that his presence is more injurious than useful if much time must elapse before he can render her any. Halting for a moment between two opinions, he makes a last examination, is convinced of a sufficiently capacious pelvis, and no impaction; he assures her of a happy issue, administers the "tincture of time," and takes leave for a little. In doing so he is unable to tax himself with any indifference towards the sufferer, his diagnosis being based upon his knowledge of touch; and his prognosis upon the previous history of the case or progress made in the time past, without any reference to the state of the woman's frame or her position.

When relieved from his jurisdiction she may perhaps seek for a mitigation of her sufferings in a change of position; and if she turn upon the right side, the pelvis being then brought into the axis of the uterus, labour may go on so rapidly as to terminate before the accoucheur can be found, or at least before he arrives.

When he enters and finds the woman delivered, he is unable to frame an unobjectionable excuse; he acknowledges it a very unprecedented circumstance, and hence, by the patient's friends, he is accused of ignorance of his profession, or indifference towards his patient. Many sudden and unexpected deliveries have occurred, and occur daily, even after hours of protracted suffering, and these, too, in well-formed women; but how much more likely in the case just related? In all such the cause of delay in the first instance is the uterus acting in a wrong direction, and the speedy delivery, in the second place, is the effect of position in adapting the axis of the pelvis to that of the uterus.

Wigand of Hamburgh remarks, that "the more completely the *os uteri* is opposite the fundus, and the more the axis of the uterus corresponds with that of the pelvis, the sooner are the pains, *cœteris paribus*, capable of dilating the *os uteri*."—(Vol. ii. p. 273.)

The late Dr Gooch, in speaking of rigid, soft, flabby, and œdematous *os uteri*, after giving the treatment for such, says, (page 187,) "do not suppose that every slowly dilating *os uteri* is to be thus treated; the delay may be for the reason, that the uterus is acting in a wrong direction, when this method is of no use."

Now, if the uterus acting in a wrong direction be a cause of protracting labour, and if the above fact, stated by Wigand, accelerate labour, how are we to guard against the one, and insure the other? for we must infer from both quotations that cases may occur in which, from some cause or other, the *os uteri*, fundus, and brim of the pelvis are not in each other's axes.

So much has been said against the practice for, and doctrine of obliquity of the uterus, as propounded by Deventer, that it is now almost as universally decried as it was formerly insisted on, when taught in every medical school in Europe.

I do not mean to revive a doctrine, the practice founded on which must, under varied circumstances, have proved so injurious. Yet, after all, it is a curious fact not to be lost sight of, that while obliquity of the *os uteri* is all but denied now-a-days, it still retains a place among the catalogue of causes protracting labour. And it can scarcely be supposed that observers so accurate as Rœderer, Peu, Levret, Velpeau, and others, would have fallen into the same mistaken idea of Deventer, had no such obliquity been found to exist. The original cause, however, has been overlooked, viz. the position of the woman altering the position of the pelvis, and hence the uterus in reference to it is found oblique.

Denman admits obliquity of the *os uteri* to be a cause of protracting labour, yet, says he, " it is not a general cause of difficulty, but an accompaniment of some other primary cause. This, when the pelvis is distorted, the *os uteri* is found in an oblique situation, yet the difficulty of labour, as well as the obliquity, is occasioned by the distortion." He adds, " it must, however, be allowed that some labours are procrastinated by the mere oblique position of the *os uteri;* singly it may not be of sufficient importance to be the cause of truly difficult ones."

It would therefore appear, that in the absence of deformity of the pelvis, the *os uteri* has been found oblique. This, however, is not discovered until the woman is in labour; and hence accurate observers have attributed the obliquity then found to the erect posture of the human species.

Rigby says, " The uterus, in fact, towards the end of pregnancy is scarcely quite straight; the upright position of the human female renders it almost necessary that the fundus should incline somewhat to one side or the other, or forwards.

Dr F. H. Ramsbotham says. " It is certainly true that when a woman has borne a large family the abdominal muscles become relaxed, lose their tone, and cease to afford that support which the gravid uterus ought to derive from them; its fundus is thrown forwards, and its mouth is directed too much backwards against the sacrum. The upper part of the uterus has also been observed to fall to the right or left side, and the mouth to be turned towards the opposite ilium."

It is admitted that in a primipara there is scarcely any inclination of the uterus to one side or the other, or forwards; but, in multiparæ, this is said to take place to a greater or less extent, according to the relaxation of the abdominal parietes; in these, however, (according to Rigby, p. 96), " the *os uteri* is neither so high up nor so far backwards in the pelvis as in primiparæ, and is reached with greater ease; whereas the head of the child, instead of being felt in the cavity of the pelvis, generally remains at the brim until labour is more advanced."

Hence, it would appear that, notwithstanding the tendency to obliquity of the uterus in multiparæ, the position of the *os uteri* in them is more favourable than in primiparæ. Waving for the present the discussion that might be entered into on these points, and keeping more closely to the question in hand, I may remark, that, if the accurate observations of Denman be admitted, viz. "when the pelvis is distorted the uterus is constantly oblique;" and if, by position of the woman, as will afterwards appear, the pelvis may become distorted, or, in other words, its axis thrown out of the mesian line of the body, it necessarily follows that the uterus, in regard to it, is more or less oblique; hence it is that the most accurate observers have declared that the uterus is never quite straight; because a person can scarcely lie down in any one position without more or less altering the position of the pelvis from that which it bears in relation to the trunk or uterus in the erect posture.

I may further add, that, out of the vast number of cases I have had an opportunity of examining in the erect posture, by the "toucher," in the Clinique des Accouchmens in Paris, I have not met with one that had not the *os uteri* more or less in a central position; but let the patient lie down and it becomes oblique! Merriman, indeed, seems to have been aware of this fact, for, in his "Synopsis," after enumerating three species of obliquity of the *os uteri*, among which is its "inclining towards one or the other side of the pelvis," he says, this "can scarcely prove a cause of much difficulty in labour; an erect posture will, if the pelvis be well formed, speedily rectify this displacement."*

The very fact, therefore, of the horizontal posture, on either side, altering the pelvis from the mesian line, and hence producing what has been considered obliquity of the uterus, forbids my assent to the treatment proposed for original and actual obliquity, when such, however rarely, is found to exist.

"If, for example, (says Denman,) the *os uteri* be projected to the left side, she ought to rest as much as possible on the same side, and so of the right," &c. "By this method the fundus of the uterus constantly leaning or inclining to the side of the obliquity, will gradually but effectually project the *os uteri* more and more to a central position."

With this doctrine I cannot possibly agree, although I believe it to be the most received opinion of the present day. I hold, however, the very opposite opinion, viz., that if actual obliquity of the uterus exist, i. e., if the *os uteri* be found projecting towards the left, the patient should be delivered upon the right side, and *vice versa.* But assertions are worth nothing without proof; I would therefore beg the reader to examine for himself. Let him, when lying in bed upon either the right or the left side, notice whether the trunk has or has not become laterally curved; let him feel the crest of both iliæ, and he will find the uppermost overlays the lower false ribs of the same side; while the other *crista ilii* is completely separated from the false ribs of the side on which he rests. Thus the normal pelvis actually becomes hitched, and is no longer in the mesian line of the body. This effect is produced by what I have called the "bending of the spine sideways." The explanation is not difficult, since we know the mobility of the spinal column, and the two points on which we rest when lying on one side. If we look at a person from behind when thus reclining, we shall easily discover that the spine is laterally curved.

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* Here then we have the answer to the question at page 17, viz. "Has the position of the woman during labour any influence on either, i. *e.*, on uterus or pelvis?"

With these facts before us, then, we must conclude that the great benefit arising from the position to be assumed by the woman, in cases where the *os uteri* appears oblique, is not owing to the effects it has upon the uterus, but the influence it has in altering the relative position of the pelvis.

Let us, for example, suppose that we were called upon to attend a patient with such a conformation of pelvis as that described in Hamilton's "Practical Observations," viz. fig. 2 of plate 4, which he took from a skeleton in the museum of Messrs Pravaz and Guerin at the Chateau de la Muette, at Passy, near Paris. "The left brim, (says Dr H.,) formed nearly a straight line from the sacro-iliac symphysis to the *symphysis pubis.*" The transverse diameter was four, and the antero-posterior four and a half inches,—sufficient room, therefore, for an ordinary-sized fœtus to pass. Whether is it likely, in such a case, that the *os uteri* is more towards the left than the right side? It will scarcely be denied that reason favours the assertion of its being more to the left boundary of the pelvic brim, (which in the above case stood near to the mesian line of the trunk); and if the *os* be found in that position, whether is it easier to twist the pelvis, as it were, or the uterus, so as to bring the centre of the brim more under the *os uteri,* that is, more towards the left? whether, in fact, the position on the left or right side is to be preferred?

From what has been already said, it must be evident that in such a case the right side is to be preferred. In all cases where the *os uteri* is found directed more towards one side, when the woman is erect, than to the other, she must rest as much as possible on that side from which it has departed; in other words, the side to which the *os uteri* is directed must be uppermost, because I have already shown that the uterus is not so much affected by her position as the pelvis.

I submit with the greatest deference, that to imagine any good can arise by the patient resting on the side to which the *os uteri* is directed, presupposes two things. 1. That the pelvis is not thrown out of the mesian line of the body when an individual is lying on one side. 2. That the uterus is as a lever (in such cases,) of which the fundus is the heavier end, and gravitates, while the *os* is projected.

"By the fundus constantly leaning or inclining to the side of the obliquity," implies, moreover, (if any good be thus effected,) that during its contractions it must so lean or incline. Rigby, however, in arguing against the doctrine of obliquity of the uterus having any influence in determining the position of the fœtus, makes the following important assertion which bears upon the point at issue.

"The moment, (says he, p. 168,) a pain commences, the fundus moves towards the mesian line of the body, so that its axis corresponds nearly with that of the pelvic brim; as the pain goes off, so does it return towards its former oblique position."

Again, if the uterus inclined or leaned, as would the heavy end of a lever, its doing so implies the existence of a "fulcrum," which must be somewhere between the os and fundus, else the latter could not be said to gravitate, nor the former to project. And, moreover, the propelling force of the uterus must of necessity be communicated first to the fulcrum, and secondarily to the *os uteri*, and labour thus (according to Wigand) protracted. Farther, we must determine where this fulcrum exists before we can accurately demonstrate the supposed good effected; and from all we know of the anatomy of the parts, we must suppose it to be very close indeed to the *os uteri*. Supposing it to be one inch from it, (and it cannot even be so much,) and the uterus from os to fundus thirteen inches, at the full period of gestation, the latter must gravitate through a space of twelve inches to elevate or project the former one inch. Again, if it thus gravitated, the intestines, which always lie behind and at either side, must become displaced, and more or less occupy the front, a thing of the most rare occurrence.

Were the uterus subject to fall backwards or forwards or to either side, according to the position of the woman, however gradual the gravitation might be, her life would be one of wretchedness and misery. Nay, even in pendulous bellies the uterus is not found to gravitate as might be expected when the woman is placed upon her back; and hence we have authors urging the necessity of applying a bandage to the abdomen in such cases, and this not being sufficient, the *os uteri* is said to have been dragged forwards.

Were it a fact, that when the *os uteri* "projects backwards" the position on the back would favour its being "gradually and effectually projected towards a more central position," we would not have the evil practice of dragging it forward taught, and originating in a country where the chief position under all circumstances was the back, both on a couch and in the labour chair.

I do not mean to assert that the back is not the best position in the case of pendulous bellies; but let any one reason for a moment upon the effects of having the shoulders raised by pillows, &c., as is generally the case when this position is assumed under those or any other circumstances; is it not to bring the pelvic brim more into the axis of the trunk? And if, previous to this position, we found the *os uteri* directed towards the sacrum, it cannot be denied that these are now still more opposed to each other. Because, if the *os uteri*, in a pendulous belly, be directed towards the sacrum, when the pelvic brim is in its usual inclined state, it must follow that it is still more so when the brim is brought nearer to right angles with the trunk. Hence the good expected from the position on the back is not obtained; hence we are recommended to apply a bandage; hence the evil practice of dragging the *os uteri* to a more central position has been thought necessary; hence it may be that certain abnormal presentations of the child, to be afterwards noticed, are produced, although they have been attributed to some "internal cause, yet unknown."

I trust I have advanced just reasons for the uterus under other circumstances appearing oblique; when in reality it is the pelvis that is thrown out of the mesian line of the body; and that this may be effected by the position of the woman, even in the absence of any deformity, must be equally clear.

In all cases, therefore, of actual obliquity of the uterus, (except in very extraordinary cases of pendulous bellies, such as those related by Dr Dewees, where it was possible to lift the abdomen with the hands,) the indication is to cause an obliquity of the pelvis, so as to bring its brim directly under the *os uteri*; it is thus and thus alone we can adapt the axis of the one to that of the other.

The question rests here, whether by position, (in ordinary cases), have we the power of altering the axis of pelvis or uterus most?

No one will deny, from what has been already said, that the pelvis is more under our control than the uterus; nor that the chief means of adapting the axis of the one to that of the other, depend on the mobility and flexibility of the spinal column, through which the position of the pelvis in reference to the trunk and uterus can be materially altered.

I do not deny that position has any effect whatever upon the uterus, but certainly not in the way, nor to the extent supposed by many; to believe so would be to imagine that it was not endowed with that resiliation possessed by every living body; but a sort of loose mass, without being supported on either side by the now distended broad and round ligaments, (those of the one side counteracting those of the other,) behind by the compressed intestines, and in front by the gradually distended integuments of the abdomen.

In fine, were the uterus to swing about as on a pivot, we should find the *os uteri* more directed towards the sacrum in multiparæ than in primiparæ, which, according to Rigby, as quoted above, is not the fact. Never does the uterus become flexed and moveable except when the child dies, and then, indeed, does it tumble down, as it were, to that side which is lowest. " A woman, (says Rigby, p. 65,) who is pregnant with a living child, feels nothing of the sort; she may dance and jump, and yet she feels no more of a living fœtus than she does of her own liver or spleen." As well, therefore, might we say that these latter changed their position in various attitudes of the body, as to affirm that the uterus containing a living fœtus did so under similar circumstances.

From the foregoing arguments it is clear that we cannot lay down exclusively any one position in which to place the parturient woman that can be applied in every case, and under every circumstance. That no single position is without objection, is proved by the fact of every country, and different parts of the same country, having some one position peculiar to it; as if, judging of the evils arising from certain positions, that which seems least objectionable is adopted.

I have already hinted that each and all of these, under certain circumstances and varied conditions, may be useful, and there is no fact that more fully bears me out in this assertion than the following, viz. the most experienced practitioners have found that the less a woman is confined to any one position during the first stage of labour, the more rapid is it. Were it otherwise, or were it a fact that by placing the woman on her left side, as most authors affirm, we more directly bring the uterus and brim into each other's axes; then I say we should at once, even at the commencement of labour, place her in that position; for experience has taught us the truth of Wigand's remark already quoted, viz. " the more directly the *os* and *fundus uteri* are in the axis of the brim, the more rapidly is the *os uteri* dilated." But Rigby says, regarding the first stage of labour, that the less a woman is confined to any one position, the more rapidly is the *os uteri* dilated; hence it must follow in the absence of further argument, that the position on the left side does not bring the *os* and *fundus uteri*, or, in other words, the axis of the uterus into the axis of the pelvic brim.

It must be remembered, that I am not bringing forward these arguments wholly to condemn the position on the left side, but to prove that it is improper to have it so stringently forced upon the minds of young practitioners, as it commonly is, without, at the same time, explaining the nature of other cases which may come their way, in which this position would be injurious, not only in the first but also in all other stages of the same labour.

Again, regarding other positions, Merriman says, " the sitting posture, after the head of the child has descended through the superior aperture of the pelvis, sometimes produces a mechanical obstacle to speedy delivery."

A gentleman of considerable experience in midwifery, with whom I have lately conversed, told me that he attended a woman whose labour had been protracted for several hours after the head had passed the brim, but on " her assuming the sitting posture the child was instantly born ;" thus what appears an injury under one circumstance proves an actual benefit under another.

It is however asserted, and not without reason, that the sitting posture favours rupture of the perineum. Those who hold this opinion affirm that it is most frequent in countries where this position is adopted ; but though it may be true that laceration of the perineum is more general in those countries, it is not so much the mere effect of the position, as the fact of the patient having more power in using the abdominal muscles in this than in any other position, by the assistance of which the child may be forced through the external parts before they are properly dilated ; for if the trunk be kept quite straight in the sitting posture, there is no alteration either in the pelvis or uterus from that which obtains when the body is erect. Of this fact any one may convince himself by standing erect and bringing one of his thighs to right angles with the trunk. In doing so he will find no change whatever produced in the position of the pelvis ; and the sitting posture is nothing more than resting on both *tubera ischii*, while the thighs are made to form an angle with the trunk, more or less approaching a right angle ; hence it is that women have been delivered equally safe in both the erect and sitting postures, though to some it has appeared unaccountable. It is, however, very natural for one to bend forwards and allow the spine to project and become convex posteriorly when sitting ; the immediate effect of which is to bring the *crista ilii* in contact with the ribs on each side, and of course the natural inclination of the pelvis is consequently completely altered ; thus by changing the position of the pelvis, and causing the uterine efforts to be directed in a line towards the perineum, it may be ruptured or labour retarded in the sitting posture.

On the other hand, where labour is known to have been accelerated by the woman " assuming the sitting" from the horizontal posture, we must either believe that, by her resting on one side, the pelvis had been thrown out of the mesian line of the body in a lateral direction, as I have in a former part of this paper endeavoured to demonstrate ; and, consequently, though the head had descended through the superior aperture, the efforts of the uterus directed it in a line towards one or other ischium ; or that (conjoined perhaps with this condition), the pelvis had been of that form, already described as having the plane looking directly forwards, which is always the case when the spine is very prominent near the sacrum anteriorly ; so that, by sitting up, the pelvis, in the first place, was brought into the mesian line of the body, and, by bending forwards, its inlet, in the second place, was brought more into the axis of the uterus ; hence it is that, when such a conformation exists, the sitting posture, or the back half-lying half-sitting, is the best to favour speedy delivery.

In Paris women are usually delivered on the back ; this position is also adopted in many parts of Germany. In Heidelberg, however, I found the position on the side most frequently adopted by Professor Naègele. In Bonn, again, the back is preferred, where, through the kindness of Professor Kilian, I had an opportunity of examining how they proceeded. The woman is placed on her back upon a narrow half couch, to the frame-work of which two supports are appended for the feet to rest upon, by which she has the power of more effectually assisting the uterus by the abdominal muscles. I believe that the assistance thus afforded may be one cause of the inutility of the position. In Dresden, again, they use a mattress with a hole in the centre, on which the woman lies upon her back. The hole is more for the purpose of conveying away the discharges than for affording the buttocks a resting place. There is one form of pelvis, however, in which, if the buttocks be allowed to rest in the hollow while the body is stretched backwards on the remaining level surface, this position is more applicable than any other ; I mean that form whose brim is less inclined than natural. Thus, by the nates being placed in the hollow, while the lumbar vertebræ rest upon its rounded edge, and back stretched upon the level surface of the mattress, the axis of uterus and pelvis may be adapted to each other.

The same effect can be produced by the patient resting on her knees on the floor with the elbows placed upon a chair, so that the shoulders and buttocks become the most prominent points.

I delivered two women in this position, who, in despite of my remonstrance, would take no other. One of the labours was rapid and easy; the other was very much protracted: the head was two hours resting on, (and partly distending,) the perinæum without making scarcely any advance. I was astonished at the delay, as the pains were powerful. She remained all the time on her knees, nor could I by any persuasion get her to take another position; before labour terminated she become delirious; she repeatedly told me she felt me keeping back the child, when in reality I was not within reach of her. In the last agony of tremendous suffering several opprobrious epithets were applied to me, of which she had not the slightest recollection afterwards; nor was the child born until she flexed the thighs upon the legs, so that the hips came in contact with her heels, the toes being at the same time rested on the floor; and thus, in the act of her crying out, "Cut it away from me," the head cleared the perineum. There was scarcely room between her and the floor for the exit of the child, and the difficulty to me was very great, for I could only reach it by one hand before and another behind, altogether the most awkward and disagreeable position in which I have ever been placed, yet it is easy to see the rapid benefit this sudden change of position effected; before which the brim of the pelvis was made to look almost downwards to the floor, and by reasoning a little on this position, we shall find that the uterine efforts were first directed in a line towards the pubis, thence backwards, (or upwards, considering her position,) towards the coccyx, requiring a third impulse forwards to the *meatus externus*, so that the propelling force of the uterus was expended before the head received any impulse in the direction of the pelvic outlet. But when the brim was brought more into the axis of the uterus, by the change of position above described, the force being directed in a single instead of a double curved direction, *i. e.* the first impulse being in a direction towards the coccyx, the head received the immediate secondary impulse in the direction of the outlet, and was thus suddenly forced through the external parts, at a time when as little expected as it was an hour or two previously.

In Britain the back was a favourite position. This was gradually changed to the sitting, which in many places is yet the favourite posture of the parturient woman; but that upon the left side has now become almost universal in this country.

It is most compatible with the woman's delicacy to have her back towards the accoucheur. It matters not how this is effected. In the Parisian Medical Society, I already expressed the opinion that the position on the back, independent of other difficulties and disadvantages, is incompatible with female delicacy.

Baron Dubois, however, very justly remarked that this idea should not allow us to adopt a wrong position under a pretended sense of delicacy, when we had it in our power to enforce that most compatible with the woman's safety.

With the principle of this sentiment I perfectly agree, *i. e.* we should not adopt any single position to the exclusion of all others when the safety of the woman becomes a question. Should there be any one, however, less objectionable than another for general adoption, I believe it to be one midway between the positions on the side and back,—resting on the dorsum of the ilium and scapula, instead of more directly upon the shoulder and side of the pelvis or back. This position has the advantage of obviating to a certain extent that obliquity or hitching of the pelvis which I have shown is occasioned by lying more completely on the side: the patient is, moreover, sufficiently turned away from the accoucheur, and while the under leg lies at ease flexed and flat upon the bed, the upper one, by being flexed so as to rest upon the sole of the foot, can be so retained for any length of time without weariness, by which the necessity of placing a pillow between the knees, as many recommend, is obviated.

I must not, however, omit to mention that some authors do not see the utility of separating the legs, and affirm that it favours rupture of the perineum, which is obviated by keeping them together without retarding labour. Against this theory may be advanced the fact that the legs may be considerably separated without the fourchette being put upon the stretch. Any one may satisfy himself on this point at once by paying a visit to the Lock Hospital, where he will often see no small degree of difficulty experienced in introducing the speculum, with all the separation of the thighs possible. Nay, I have seen it more easily introduced by Dr Churchill in Dublin, when the woman's thighs were not separated at all, but she lying on her side, in the usual position women take in labour.

It may now be said by those who advocate the propriety of keeping the legs together in labour, that, if the speculum be thus more easily introduced, the child ought for the same reason to be in proportion as easily born without separating the thighs; but, if we consider that the occiput first, and afterwards the shoulder, ought to be brought in close apposition with the pubis as the rest of the body makes its transit, we shall see just reasons why the thighs not only ought, but must be separated in actual parturition. I fear this is a slight digression from the points I have in the foregoing pages been discussing, which went to show the impossibility of fixing on any one position for the parturient woman applicable to all cases. I have given the least objectionable one, yet it must also vary according to circumstances, in diagnosing which, we must be guided wholly by our knowledge of what is the standard conformation of body and pelvis, and judge of any deviation that may exist by the varied circumstances connected with parturition when applied to the case in hand.

Thus, in a protracted labour, when we find the pelvis capacious and the condition of the soft parts such as to warrant the anticipation of a speedy delivery, having the pains sufficiently powerful, yet some unaccountable delay, we may justly infer that the birth of the child is protracted by the pelvis and uterus not being in each other's axes; or, in other words, that the woman is not in a proper position, to rectify which can only be judged of by existing circumstances.

Thus it will often happen that a little common sense may go far in correcting positions that may exist in the rules by which any practice, society, or profession is governed. And though what I have in the foregoing pages advanced may have little to recommend it at first sight, yet I have the hope that it may lead to important results in midwifery, when taken up by more able and accurate observers.

Having exhausted my present limits, I shall reserve for a future communication, the causes of rupture of the uterus, the arguments in favour of its being sometimes produced by a mal-position of the woman, as well as a deviation in the mal-position of the child being likely sometimes to depend upon the same cause, &c.

From what has already been said the following may deduced :—

1. That no one position can be applicable to all cases.

2. That the axis of the pelvis may be materially altered by the woman's position.

3. That any change in the position of the pelvis, produced by the position of the woman, must be effected by the bending of the spinal column backwards, forwards, or to either side.

4. That there may be lateral curvature of the whole spinal column, or curvature of the superior dorsal vertebra antero-posteriorly without altering the axis or inclination of the pelvis in reference to the whole trunk and centre of gravity.

5. That we cannot have curvature of the lumbar vertebræ antero-posteriorly without very materially altering the inclination of the pelvis.

6. That the brim of the pelvis may form a much more acute or obtuse angle with the spinal column than the standard, even in the absence of disease.

7. That when either of the latter conditions exists, we can, by bending the spinal column backwards in the one and forwards in the other, adapt the pelvis to the axis of the uterus.

8. That this power of altering the pelvis upon the spine, so as to apply to midwifery, exists only in the mobility of the three or four superior lumbar, and four or five inferior dorsal vertebræ.

9. That if any single position could be fixed upon for the parturient woman, that midway, between lying on the back and side, is least objectionable.

ART. VIII.—*Secretion of Carbon by Animals.* By ROBERT RIGG, F.R.S.

To the Editor of the Edinburgh Medical and Surgical Journal.

SIR,—The scientific world is at present much occupied with the application of chemistry to animal and vegetable physiology, and it may be interesting to some of your readers to know, that, by a few very simple experiments, they may satisfy themselves upon that branch of the subject which relates to the secretion of carbon by animals.

Suppose an animal, which comprises in its whole system 50 parts by weight of carbon, to be kept for five days, during which it consumes other 50 parts, it is evident that if, during these five days, it gives off to the surrounding atmosphere 60 parts, and at the end of that time is found to have increased its weight of carbon 10 parts, there is a positive gain of carbon equivalent to 20 per cent.

The experiment may be easily made upon young small animals. Take two of these so nearly alike that there can be no material difference in the weight of carbon they comprise. Kill one of these, and expose it to a temperature not exceeding 220° for two or three days; it may then be powdered, and by subjecting an average sample to analysis with oxide of copper, the weight of carbon comprised in the entire animal may be determined with the greatest certainty. The other being supplied with food, the weight and chemical constitution of which is ascertained, should be kept in a limited atmosphere, which must be changed and tested every one, two, or three hours.* The proportion of carbonic acid in that atmosphere will show the quantity of carbon given off by the animal in the course of the experiment, and the increase or decrease of carbon in the animal itself may be ascertained in the manner above mentioned.

In this manner I have experimented upon many animals, and without taking account of the carbon which passes off otherwise than by respiration, the results have invariably been a great increase of carbon; an increase which cannot be accounted for unless we conclude that carbon is secreted by animals.

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* The health of the animal appears to be affected by an atmosphere containing more than 5 per cent. of its volume of carbonic acid.

Amongst my best experiments are those made with young mice. A healthy young mouse, weighing 200 grains, comprises in its constitution from 25 to 30 grains of carbon. When fed daily with 60 grains of bread moistened with water containing about 16 grains of carbon, it increases in weight and imparts to the atmosphere from 20 to 26 grains of carbon, this quantity varying generally with the quietness or the activity of the habits of the animal. A kitten, from six to ten weeks old, when supplied daily with four fluid-ounces of skim milk containing 66 grains of carbon, will increase in weight, and impart to the atmosphere from 80 to 100 grains of carbon.

Either of these animals may be kept without food until they give off by respiration a weight of carbon equal to 80 per cent., and retain from 60 to 70 per cent. of that which they comprised at the commencement, shewing that a weight of carbon equal to 40 per cent. has been secreted. A tomtit was placed under experiment without food: the bird was violent and restless during its imprisonment. In sixteen hours it imparted to the atmosphere 65 per cent. of carbon, when it appeared to die of exhaustion, and retained 77 per cent. of the weight of carbon it originally contained, shewing a secretion of carbon in sixteen hours, when under violent exertion, equal to 42 per cent.

On making the carbon in the food and that in the air respired by a full-grown person the basis of our calculation, we obtain results which favour the conclusion that carbon is likewise secreted by man. Physiologists estimate the weight of carbon in air respired by an adult at from 5000 to 6000 grains per diem. I have subjected to ultimate analysis many articles of food, and find this weight of carbon far exceeds that in the food consumed by most labouring men, who may be supposed to impart to the atmosphere the greatest weight of carbon. A person eating each day after the following rate will consume 6000 grains of carbon:

Rump steak,	1	pound, containing		1050	grains of carbon,
Bread,	1½		-	2830	—
Potatoes,	½		-	310	—
Porter,	2	pints,	-	760	—
New milk,	2	fluid-ounces,		57	—
Butter,	1½	ounce,		320	—
Cheese,	1	do.	-	150	—
Sugar,	2	do.	-	350	—
Coffee,	1	do.	-	96	—
Tea,	1	do.	-	80	—
				6003	—

This weight of carbon is not more than is consumed by some persons who are actively employed, but it very far exceeds that in the food of our labouring population; and on comparing it with that allowed for each adult in the different workhouses, &c., we have, in the dietary of the

City of London Union, -	75	per cent. of this 6000.
Brentford Union, - -	50	do.
Uxbridge, - - -	55	do.
Alesford, - - -	56	do.
Macclesfield, - - -	44	do.
Westminster New Prison, -	57	do.
Millbank Penitentiary, - -	80	do.
House of Correction, Clerkenwell,	53	do.
Hanwell Lunatic Asylum, -	75	do.

And if we make the carbon in the food of some of our agricultural labourers the subject of comparison, we find the deficiency greater than in any of the above-mentioned dietaries. I would add to these many other experiments which furnish, in my opinion, irresistible evidence of the secretion of carbon by animals. If an animal be kept without change of circumstances, or of diet, except as to the quantity of food, it will be found that the weight of carbon in the air respired does not vary in proportion to that consumed in the shape of food; on the contrary, the deficiency of carbon supplied seems to be met by an extraordinary effort of the animal system, as appears from the following results of accurate observation.

	The food.	The air respired.
If an animal in the first 24 hours has a plentiful supply of food, there is in	80 grs. of carb.	100 grs. of carb.
In the next 24 hours a less quantity,	70 do.	94 do.
Do. - sparing do.,	60 do.	87 do.
Do. - small do.,	50 do.	78 do.
Do. - very small do.,	40 do.	65 do.

But if the animal, instead of having its quantity of food varied, should be sometimes left in a quiescent state, and sometimes excited to great activity, the weight of carbon given off will be found to vary in proportion, within certain limits, to the activity of the habits of the animal, and the exertion called forth.

If the carbon in the food be represented by -	100
That given off by an animal of easy habits will be	110
Do. do. active do. -	130
Do. do. when under exertion,	140
Do. do. great do.	150

If the animal be both stinted in food and excited to great activity, the difference between the carbon comprised in the food and that given off by the animal is as follows:—

	In the food.	Given off.	Difference.
Carbon, - -	100	120	20
Do. - -	80	105	25
Do. - -	60	90	30
Do. - -	50	85	35

Hence, food is a substitute for expenditure of animal strength. To this may be added, that when an animal is distressingly exercised, the weight of carbon in the carbonic acid given off by respiration is at first increased, afterwards gradually diminishes, and becomes much less when the animal is in a state of exhaustion.

Rest alone is not then sufficient, but rest and food soon restore the strength of the animal, and with its strength its power of secreting carbon,—a power which I conceive to be essential to animal life, and which will probably furnish a solution to some of the most difficult problems of animal physiology, including that of the generation of animal heat. I am, Sir, your most obedient servant,

Robert Rigg.

Greenford, Middlesex,
August 15, 1844.

Art. IX.—*On the Colour and Structure presented by Corpora Lutea in the early stage.* By Robert Paterson, M.D. &c., Leith. See Plate VIII.

Case I.—A respectable and very fine-looking young female, about 19 years of age, committed suicide under most painful circumstances.

It appeared from the account given by the relations, that the deceased had been very much attached to a young man who visited the house, and with whom they feared an improper intimacy existed. The deceased had menstruated about a month previous to her death, and precisely three weeks before that event she had remained out till very late in the company of the young man referred to, and for which his visits to the house were prohibited. It was distinctly stated that the deceased had never seen him from that night. The suicide was committed about 11 o'clock at night; and an opportunity was afforded of examining the body next morning. The uterus and its appendages were carefully removed for examination afterwards, and with which I was assisted by my friend, Professor Allen Thomson.

A general glance at the uterus and appendages at once suggested the remark that they were plump and rounded in form, more so, indeed, than I had ever previously witnessed them. The uterus was slightly larger, and certainly more rounded in its appearance than the natural unimpregnated organ. Its mouth was completely filled up with thick, tenacious, but clear mucus. Upon cutting into its cavity, through the anterior wall, it was found to be lined with a very delicate decidual membrane. This membrane lined the cavity of the uterus throughout, but did not descend either into the neck or orifices of the Fallopian tubes; it was of a delicate flesh colour, with slight irregularities on its internal surface, giving the appearance of gentle elevations and rugæ or furrows between them, in fact resembling much the furrows produced in the palm of the hand by gentle flexion of the fingers. It was adherent to the internal surface of the uterus, could with difficulty be separated from it, and was a little firmer in texture to the feel than could be expressed by pulpy. Its minute structure beneath the microscope displayed the characteristic tubules of the decidua. No ovum, although most minutely and carefully looked for, could be detected.

The lateral ligaments and Fallopian tubes were about an equal length on both sides; but the left ovary was twice the size of the right. Both ovaries had throughout much of their natural external appearance, with the exception of the extremity of the left ovary, nearest the uterus. About the centre of this enlarged extremity was a slightly depressed bluish point of irregular shape, and without presenting any mark like a cicatrix. Shining through the enveloping membrane of the ovary, immediately surrounding this blue spot, was an orange-coloured substance, which again was surrounded with patches of minute vascularity, and one or two small points of extravasated blood. (Plate VIII. Fig. 1.)

Upon bisecting the ovaries their stroma was observed to present a very similar appearance, with the exception of that more immediately in contact with the large orange-coloured body at the uterine extremity of the left ovary. Here the stroma appeared more gorged with blood and more condensed in structure than the other parts. The orange-coloured body itself presented an irregular oval shape about three-quarters of an inch long and nearly as much broad; the ovary being respectively two inches long and about one inch broad. Its colour was orange, with here and there patches and striæ of bright crimson. One irregular dark-coloured patch ran nearly through its whole length, and sending dark-coloured though delicate prolongations into the folds of the orange-coloured substance. A smaller patch of a similar description also existed. The structure of the yellow body was distinctly, though irregularly folded, and at the outer surface each fold was marked by its rounded extremity, and by a crimson-coloured line running between them. (Plate VIII. Fig. 2.)

There is every reason to believe that this unfortunate young woman was impregnated on the night of her long nocturnal absence from home. Her having just ceased menstruating a few days after this occurred, and her never having seen the young man afterwards, render this almost certain; and yet, with the most careful examination, no ovum could be detected. This case appears to me most important. It furnishes us with data as precise as we ever can expect to obtain in practice; and it presents us with the appearance of the parts twelve hours after death, and of course quite unaltered, both in colour and texture, from the results of decomposition. From it, therefore, we may depart as from a fixed point, in comparing others in which data are awanting, and often, of course, where we have no means of judging of the duration of pregnancy. It was obvious that this *corpus luteum* had at a earlier period presented a very different appearance. The central dark-coloured part bore all the marks of a central puckering like a cicatrix, and the bloody tinge which the yellow structure presented, coupled with the numerous red-coloured striæ and small patches of extravasated blood throughout its substance, and in the stroma surrounding it, at once suggested the idea of its formerly having possessed a dark-red colour throughout, which colour was now just passing away into the dull yellow-coloured substance usually described as being characteristic of the true *corpus luteum*. It appears to me that this latter circumstance is a most important one as regards the early stage of *corpora lutea*, and one which some pathologists do not seem to have observed, or even to have given credence to. The deep tint of colour observed in this case, when compared with the next I am about to mention, and which, from the consideration of every circumstance connected with it, must have been of an earlier date than the one just related,—this I say, when compared, on the other hand, with my third case, where impregnation was so much farther advanced that a small fœtus was found, leads us distinctly to observe the gradual transition which takes place from the red colour of the very early *corpus luteum*, to the dull yellow of the more advanced.

Case II.—A woman, upwards of 30 years of age, was supposed to have been murdered by her husband in a fit of jealousy. It appeared that the parties had been married eight years, and had had no family. The deceased had been of dissipated habits, and latterly had contracted an acquaintance with a young man, whose frequent visits to her in the absence of her husband excited suspicions against them. No accurate information could be given regarding her last menstrual periods. The uterine organs were removed at the inspection, and examined afterwards.

The right ovary presented externally an enlargement of the size of an ordinary marble, of a dark purple colour, having several minute vessels ramifying on its surface, a distinct depression in its centre, and the appearance of a minute cicatrix. Upon making a section of this ovary, blood exuded abundantly from the whole of its cut surface. The stroma of the ovary, immediately around the *corpus luteum*, was very much filled with blood. The *corpus luteum* itself was of an intensely dark colour around its external margin; this dark margin being thicker, however, on one side than the other. The external membrane, which occupied the centre of the mass, was very ragged and irregular in shape, of a bluish colour, and partially filled with blood. Between it and the dark external margin formerly mentioned the substance was of a brighter red colour, having dark striæ or folds running through it. After immersion in spirits these striæ assumed a yellow colour, and the internal membrane could (with delicacy) be lifted up from the substance of the *corpus luteum*. (Plate VIII. Figs. 3 and 4.) The uterus was slightly enlarged, and contained an extremely thin and soft decidual membrane. It was remarked regarding this case in a former Number of this Journal, that it appeared to represent the *corpus luteum* at a very early stage; indeed, shortly after the escape of the ovule from the Graafian vesicle. In this case the minute structure and general appearance of the section of the *corpus luteum* bears a close resemblance to that described in Case I. Its texture, however, was much more gorged with blood, and the blood in the centre of it had quite the appearance of a recent clot. The striated or folded structure was just becoming developed, and, in fact, the characteristic structure of the true *corpus luteum* was as much present as in the first case, it only being in this last at an earlier period. On making a section of the ovary of a case similar to the above, the first glance at the *corpus luteum* would give very much the idea of a clot of blood; upon more minute examination, however, it is found to possess a peculiar and characteristic structure.

The case now related was formerly published by me in the 142d Number of this Journal, and again by Mr Wharton Jones in the Medical Gazette for August 9, 1844. It is one, however, of so much interest and importance that I need make no apology for republishing it in this place; more especially as Dr Lee of London had at one time called in question the truth and accuracy of the observation. It is decidedly the earliest true *corpus luteum* on record, and exhibits the deep-red tint of colour presented by this structure at a very early period

From causes of which I am ignorant, Dr Lee, in his Lectures in the Medical Gazette, designated it a mere "clot of blood." In several letters which were published in that Journal, I endeavoured to explain to Dr Lee that this opinion must have arisen from some oversight; and I brought the testimony of various professional men, and more especially of Dr Allen Thomson and Dr John Reid, to prove that they considered it a true *corpus luteum*. Dr Lee, however, in stronger terms than ever, still adhered to his former opinion, and declared that "the said clot of blood did not present one of the characters of a true *corpus luteum;*" and that, if he "was summoned into a court of justice, he would have no hesitation in declaring upon oath, from the evidence furnished, that the proofs of pregnancy were wholly wanting." This was certainly strong language to use regarding a specimen which I had, after careful and minute examination, described and actually delineated as a true body, and had, in addition, brought forward strong testimony in support of my views. I was still in hopes, however, that the arguments used, and authority brought forward, would have had its proper weight with Dr Lee, and that he would modify in some degree the confidence with which he questioned the correctness of my observations, when his lectures appeared in a separate form. Instead of doing so, however, he published his remarks word for word as they had appeared in the Gazette.

Being satisfied that he was labouring under some mistake in calling in question the correctness of my observations upon this case, as it appeared in a former Number of this Journal, I consulted several medical friends as to the propriety of forwarding, for Dr Lee's personal and unbiassed inspection, the uterus and ovaries of the case in question; and I requested a medical friend, who was quite unaware of the history of the preparation he was forwarding, till some time afterwards, to send to Dr Lee the specimen in question.*

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* No. 1. *Monkwearmouth, May* 9, 1844.

Dear Sir,—I venture to encroach on your valuable time to ask your opinion of the accompanying preparation, illustrative of a point to which the medical world knows you to have paid much attention.

Is the *corpus luteum* in this case a true or false one?

It was taken from the body of a female upwards of thirty years of age. She died suddenly, and no accurate information could be gained regarding her last menstrual periods. Several medical gentlemen have examined the *corpus luteum*, and are so divided in opinion about it, that they would feel deeply indebted to you to examine it, and favour us with your decision upon the question.

I hope you will excuse the great liberty a stranger thus ventures to take: but, knowing your kindness, and having every confidence in your opinion, I have the honour to remain, your obedient servant,

Dr Lee. (Signed) Henry O. Bowman, M. D.

P. S.—As the preparation belongs to a medical friend, who was so kind as to allow me to examine it, I shall feel obliged by your returning it with your decision. If it were mine you would be heartily welcome to it: Indeed, I should have considered it my duty to have presented it to you.—H. O. B.

No. 2. *London*, 4 *Saville Row*, 13*th May* 1844.

My Dear Sir,—The preparation reached me in safety this morning. I am now unusually busy, but hope in the course of a few days to be able to examine the ovarium with sufficient care to give a positive answer to your question, which I consider to be one of the greatest importance. I remain, My Dear Sir, very truly yours,

Dr Bowman. (Signed) Robt. Lee.

No. 3. 4 *Saville Row, 22d May* 1844.

My Dear Sir,—There is every reason to believe that it is a true *corpus luteum* in an early stage, although it projects more than the true *corpus luteum* usually does.

The membrane in the centre is the Graafian vesicle, both layers of which, inside the yellow matter and coagulated blood, can be demonstrated; the thick layer outside consists of blood and of the yellow granules always present in the true *corpus luteum*. This layer is in contact with the stroma of the ovary. The whole is now undergoing a complete microscopical examination by Mr Wharton Jones, who is decidedly of the same opinion about the nature of the structure. I propose, with your permission, to retain in my possession the half of the *corpus luteum*, for the purpose of verifying the observations made, and of returning to you in a short time the uterus with the other parts undisturbed. I hope the gentleman to whom the specimen belongs will not object to this; at least, that he will not require it to be returned to him for several weeks to come, until we have had time to complete the investigation, and to give the result to the Med.-Chir. Society. If, however, he has any intention

He, in a short time, and after a careful examination of the specimen, returned an answer, declaring it to be an early true *corpus*

of publishing upon the subject, I hope you will let me know, that I may not interfere with him in any way. I remain, My Dear Sir, very truly yours,

Dr Bowman. (Signed) Robt. Lee.

No. 4. 4 *Saville Row*, 26*th June* 1844.

My Dear Sir,—I trust you will receive the preparation in safety. I have detained it longer than I intended, in consequence of some important business coming upon me. I hope in a short time to send you a copy of Mr Wharton Jones's microscopical examination of this *corpus luteum*, that your friend may give it to the public with the entire history of the case. I remain, My Dear Sir, yours very truly,

Dr Bowman. (Signed) Robt. Lee.

No. 5. *Leith*, 26*th July* 1844.

Dear Sir,—You will recollect that some time ago a correspondence took place between us, (through the medium of the Medical Gazette,) relative to the subject of *corpora lutea*. In one of your lectures which were published in that Journal, and in reference to one of the cases which I had described and figured, you used the following words:—

" The figure shows nothing but a red clot of blood, not possessing one of the characters of the true *corpus luteum*." I endeavoured to explain to you this matter more fully in a letter on the characters of early *corpora lutea*, in a subsequent Number of the same Journal. In an answer inserted by you in the Gazette, you again declared, that in the case referred to, the red clot of blood in the ovarium does not present one of the characters of a true *corpus luteum ;* " my conscientious belief is, that this woman died during menstruation ;" and you add, " If I were summoned into a court of justice, I would have no hesitation in declaring upon oath, from the evidence furnished, that the proofs of pregnancy were wholly wanting."

I offered to the Gazette some remarks in reply to those observations of yours. The editor, however, would only allow of the publication of them among his extra limites, and charged for their insertion there. But in the very next number of the Journal he published a letter containing an attack upon myself, and upon my friend Professor Reid, relative to the whole question at issue between yourself and me. This last letter was signed Scrutator, and the opinion expressed in it, the style of its writing, and the language used were so similar to your own, that at first I felt inclined to answer it as such ; but on second thoughts refrained from doing so, under the conviction that it was certainly impossible for you to be guilty of such a subterfuge. I was in hopes, however, that the arguments I had used, and the authority of Professors Reid and Thomson, that I had brought forward in support of my views of the particular case which you principally doubted, would have had some weight with you. In this I was disappointed, for on lately looking into your lectures, published in a separate form, I find your condemnation of the truth of my observations published verbatim as it appeared in the Gazette.

Under these circumstances I resolved, by the advice of some professional friends, to submit to your own inspection the preparation of the disputed *corpus luteum* itself. My friend, Dr Bowman of Sunderland, submitted it to you, (not suspecting it was the identical specimen so much discussed.) You have declared to Dr Bowman, in two letters which I have now before me, that the body which you formerly characterised as not possessing one of the characters of a true *corpus luteum*, is now really a true *corpus luteum* in an early stage ; and you then give a minute description of the body,—a description agreeing in every particular with that given by me in my original paper in the Edinburgh Medical and Surgical Journal.

" If from conviction, (I quote your own words from the Medical Gazette of 16th December 1842,) I could alter my opinion on Dr Paterson's *corpora lutea*, it would afford me the greatest satisfaction to do so ; for I know from experience that the pleasure of renouncing an error is only equalled by the delight felt on discovering a new truth." Entertaining as you do sentiments of this kind, I am sure that you will at once do me the favour of publishing, in the pages of the Medical Gazette, that the case in question has been totally misrepresented by you in your *luteum*, and requested permission to describe it as such before the Medico-Chirurgical Society of London. See letter, No. 3.

published lectures, and in your observations in the above Journal. As a young member of the profession I shall feel happy by your following this course, and thus removing from my character as an observer the uncertainty which your published remarks might be calculated to place upon it. If not, I shall consider it a duty which I owe as well to myself as to the very respectable Journal in which my paper originally appeared, to publish such a statement of the matter as may seem necessary to me.

The favour of an early reply will much oblige.

To Dr Robt. Lee, I have the honour to be,
4 *Saville Row, London.* Your most obedient servant,
(Signed) Robert Paterson.

No. 6. 4 *Saville Row, 30th July* 1844.

Dear Sir,—I shall feel the greatest pleasure in communicating to the Medical Gazette the grounds upon which I was led to conclude that it was a true *corpus luteum* which was submitted to me for examination by Dr Bowman. Before, however, doing this, and complying with the request contained in your letter, it will be requisite for me carefully to examine your description of this body in the Edinburgh Medical and Surgical Journal, and to determine how far it supports Dr Montgomery's view of the structure of the *corpus luteum.* Mr Wharton Jones, you will find, is of opinion that it completely overturns that view, which is, after all, the only question in which I feel much interest. I remain, Dear Sir, very faithfully yours,

Dr Paterson. (Signed) Robt. Lee.

P.S.—Perhaps you will have the kindness to send me copies of my letters to Dr Bowman, as I have not preserved them.

No. 7. *Leith, 1st August* 1844.

Dear Sir,—I beg to acknowledge receipt of your letter of the 30th July, and will have much pleasure in seeing published the grounds upon which you were led to conclude that it was a true *corpus luteum* which was submitted to your examination by Dr Bowman.

Before doing so, however, I may state that in your letter to me you seem altogether to take an erroneous view of the nature of my complaint against your criticisms.

It is quite obvious that two questions can be raised regarding the specimen of *corpus luteum* which I described, and sent you, viz. *First,* Is it a true or false *corpus luteum?* and, *Secondly,* If a true *corpus luteum,* does its structure favour the anatomical theories of Drs Baer and Montgomery, or of Dr Lee, regarding such bodies? With this latter question, however, I have nothing at present to do. It is the first, and the first only, of them which interests nine-tenths of the profession; it is it, and it alone, which is practically important; and it was in regard to it, and it alone, that you challenged and condemned my published observations on the case lately submitted to your investigation. But in your letter of July 30th, you evade so far this point, and add that the only question on which you feel much interest is the structure of the *corpus luteum.*

I, of course, feel interest principally in the other question, as it is in fact the only one in which the correctness of my statement as an observer and writer was doubted.

It was on this point alone that I asked for an explanation from you.

It will give me much pleasure to discuss the structure of the *corpus luteum* with you at any future time. But at present I must protest against the two subjects being attempted to be mixed up; and I have to request, therefore, from you a public explanation of your severe and contradictory statements regarding the alleged false nature of the *corpus luteum* in question, as it was on this alone that you passed on me a public condemnation.

I need not occupy my own time or yours by quoting, for the sake of comparison, either your own statement as published in your letter in the Medical Gazette, and which, you said, could be given on oath, with the contradictory one in your letter to

I next took an opportunity of writing Dr Lee, and telling him that the preparation on which he had delivered such a decided opinion was the identical specimen which he had formerly regard-

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Dr Bowman. Nor need I quote the similarity of the description as given by me in the Edinburgh Medical and Surgical Journal with that contained in your letter to Dr Bowman, and drawn from an unbiassed examination of the specimen itself. These you will yourself have an opportunity of comparing.

I have much pleasure in inclosing to you copies of your letters to Dr Bowman, and I shall expect to find your explanation in as early a number of the Gazette as is convenient. I have the honour to remain, your most obedient servant,

Dr Robt. Lee, Robt. Paterson.
4 Saville Row, London.

No. 8. *London*, 4 *Saville Row*, *5th August* 1844.

Dear Sir,—I wrote another letter to Dr Bowman before the 22d May 1844, of which you have not forwarded a copy to me. I am much obliged to you for the copies of the two sent, and I trust you will feel entirely satisfied with the account, which Mr Wharton Jones will give in the next Number of the Medical Gazette, of his microscopical examination of the body in question.

Dr Paterson. I remain, Dear Sir, very faithfully yours,
(Signed) Robt. Lee.

No. 9. *Leith*, *9th August* 1844.

Dear Sir,—I beg to acknowledge the receipt of yours of the 5th instant, which would have been answered sooner, but that I have been waiting for a reply to a letter sent to Dr Bowman, for the purpose of ascertaining whether or not there was a previous letter to those, copies of which were sent to you. I was astonished to observe your insinuation that I had kept back a letter which you wrote to Dr Bowman of a previous date to those, copies of which were sent you. In reply to this accusation I may state, that I never received any letter which you had communicated to Dr Bowman prior to the date of those, the copies of which were sent you. In addition to this, however, in your letter of the 22d May, you refer to no prior communication to Dr B., which, I think, under the circumstances, would have been referred to. I am equally convinced that Dr Bowman did not receive any previous letter, otherwise it would have been forwarded to me. I am sorry that I have not heard from Dr Bowman before this time, for it has delayed my reply to your last letter longer than I intended. I shall be glad if Mr Jones's statement of the question in dispute between us shall be satisfactory to me. I remain your most obedient servant,

Dr Robt. Lee. (Signed) Robt. Paterson.

No. 10. *Leith*, *August* 12, 1844.

Dear Sir,—I presume that you will already have had the answer from Dr Bowman himself, which I am now about to give you regarding your communications with him. Your letters to Dr Bowman were three in number. The first, dated 13th May, acknowledging your receipt of the preparation; the second, dated 22d May, containing your opinion regarding it; and the third, dated 26th June, accompanying the return of it. These, Dr Bowman distinctly states, were the only letters received from you in connection with this preparation.

I have just seen Mr Jones's description of the *corpus luteum*. It contains one or two most important errors, which it will be necessary to have corrected. Before doing so, however, I am anxious to know if Mr Jones's letter contains all the statements you propose to make public regarding your unjust criticisms on the case in question, and as stated by me in a former letter. I should feel obliged by your communicating to me, with your earliest convenience, if any further statement on the subject is to be made public. I remain, Sir, your obedient servant,

Dr Robt. Lee. (Signed) Robt. Paterson.

No. 11. *London*, 4 *Saville Row*, 12*th August* 1844.

Sir,—You have placed yourself in a position which renders it necessary for me to discontinue writing to you. I remain, Sir, your obedient servant,

Dr Paterson. (Signed) Robt. Lee.

ed as not presenting one of the characters of a true *corpus luteum*, and requesting that, as a simple act of justice, he would publish a suitable statement in the pages of the Medical Gazette. To this request Dr Lee returned for answer, that "he would feel the greatest pleasure in communicating to the Gazette the grounds on which he had been led to conclude that it was a true *corpus luteum*." No statement, however, has yet appeared from Dr Lee, although such was requested and such promised. Mr Wharton Jones, as I have already stated, published an account of the examination of the specimen in question, and declared it to be an early true *corpus luteum*.

It will be at once obvious to every unbiassed member of the profession that Dr Lee has found the proofs in favour of the specimen, as delineated and described by me as a true *corpus luteum*, and which he represented as merely a clot of blood, too strong to resist; yet he has hitherto made no public admission to this effect. Thus, as will be seen from the correspondence given in the notes, that, after declaring his positive opinion on the true nature of the *corpus luteum*, he first attempts to get clear of the matter, by declaring that my case bears out his opinion of the minute structure of the *corpus luteum*, "which, after all, is the only question on which he feels much interest;" he, secondly, declares, that he at first considered it a mere clot of blood, but that Mr W. Jones, "on whose judgment he places much more confidence than on his own," considered it a true body; and, after all these attempts fail, he declares that there was another letter to Dr Bowman, (the gentleman who sent the preparation for me,) more than those the copies of which I transmitted to him. No other letter, however, was ever sent by Dr Lee than the three, the copies of which are annexed. Such is Dr Bowman's distinct statement; such is borne out by the letters received, none of which bears the slightest reference to any other, and which, seeing the closeness of the dates, it certainly would have done; and such is borne out by Dr Lee's conduct in the matter; for while he fancies that another was sent, he seems to have no copy of such, and to have a total forgetfulness of any idea or matter it contained.

I appeal with confidence to the intelligence of our profession, to say whether Dr Lee has acted justly or fairly with me in this

No. 12. *Leith, 14th August* 1844.

SIR,—The tenor of your letter received this morning did not surprise me. It was the course I was prepared to expect you would pursue after equivocation had failed you. It now only remains for me, in vindication of my character, to publish the correspondence that has passed on the subject, with such notes and statements as may appear necessary. Before doing so, however, I will still have the patience to wait one or two weeks, in hopes that you may be advised yet to make the necessary public reclamation in the pages of the Medical Gazette.

I remain, Sir, your obedient servant,
(Signed) ROBT. PATERSON.

DR ROBT. LEE.

matter. My observations on the *corpus luteum* had been accredited and republished in various British and Continental Journals, until the accuracy of my very first case was called in question by Dr Lee; and, even after a personal and completely unbiassed examination of it, though he has declared that I was right in considering it a true body, yet he has not made this acknowledgment in the proper mode. I am the more astonished at Dr Lee in this matter, seeing that he formerly so decidedly stated his opinion, that "if from conviction he could alter his opinion upon my *corpus luteum*, it would afford him the greatest satisfaction to do so;" and it must appear strange, that although he has altered his opinion, he will not acknowledge it.

The foregoing remarks I have felt it my duty to make, solely in order to substantiate the accuracy of my own observations on a point which I acknowledge I regard as highly important in elucidating the history of embryology. I feel that the three cases now adduced afford a series of phenomena most important in determining the true characters of the *corpus luteum* in its early stage, and elucidating the changes undergone at that period. I have studied to avoid saying any thing disagreeable to Dr Lee, for whose character I feel high respect; and I am not without hopes that, if he considers the matter dispassionately, he will allow that I have only given the explanations which he would in similar circumstances have done.

CASE III.—The next *corpus luteum* which I have to describe was taken from the body of a patient of my friend, Dr Watson of Leith, and which has already been described by him in this Journal. The patient died of the hæmorrhage arising from the rupture of the Fallopian tube from tubal pregnancy. The fœtus was about the size of a pea, and therefore the patient might have been six weeks or two months pregnant. The *corpus luteum* was of a bright yellow colour around the circumference, its anterior being filled up with a gray fibrinous-looking coagulum, in the middle of which was a central cavity containing fluid. Although no strict measurement of the size of the *corpus luteum* was taken at the time, it appeared to be a large one, nearly as large, I should say, as in my case first. It presented, however, no red colour whatever in its structure, and the folding of the yellow matter itself seemed to have undergone considerable condensation or closing up in the structure. It had, in fact, all the appearance of having been simply a little further in advance of the first case, and it well points out the great changes, both in colour and structure, which take place in the *corpus luteum* even prior to this early period; for after this up to the full time of pregnancy, little change takes place in its general appearance;—it only becoming very gradually of smaller size. (Plate VIII. Fig. 5.)

PART II.

CRITICAL ANALYSIS.

ART. I.—*Anatomie et Physiologie du Systeme Nerveux de l'Homme et des Animaux Vertebres.* Par F. A. LONGET, Laureat de l'Institut de France, &c. &c.

Anatomy and Physiology of the Nervous System of Man and the Vertebrated Animals, with pathological observations on the nervous system, and experiments on the higher classes of animals. By F. A. LONGET, M.D., Laureate of the Institute of France, Professor of Anatomy and Physiology, &c. &c. 2 vols. 8vo, with numerous Plates. Paris, 1842. Vol. i. pp. 942. Vol. ii. pp. 968.

SINCE the brilliant discoveries of Bellingeri, Bell, and Magendie were published, much has been done by other inquirers to elucidate various points which were necessarily left by them more or less imperfect. These discoveries, however, still form the ground-work of all that has since appeared, and several of the seemingly anomalous results at which their authors arrived have been since explained by more minute investigation into the anatomy of the anastomosing branches or origins of various filaments. It will be remembered, however, that these original experimentalists did not quite agree relative to the function performed by those nerves which originate from the posterior column of the spinal chord; for, while Bell maintained that they were simply nerves of sensation, Bellingeri attempted to demonstrate that they both regulated sensation and so much of motion as to prove antagonists to those nerves which originated from the anterior columns of the chord. We shall presently see how far subsequent research corroborates or disproves either of these theories. It is to Brachet especially that we are indebted for the more consistent views of the functions of the ganglionic system of nerves—views which have been more or less completely adopted by all late writers on the subject.

M. Longet's work contains one of the best accounts we have seen of the present state of our knowledge of the anatomy, physiology, and pathology of the nervous system; and no one appears to be better qualified for the task, as he has proved himself one of the most successful late investigators into this difficult branch of physiological science. M. Longet's work is divided into two great sections, the first treating of the general anatomy and physiology, the latter of the special anatomy and physiology of the nervous system.

The development of the nervous system forms the subject of the first chapter. Three views have been at different times maintained, relative to the development of the nervous system. 1. That the central parts were first developed, and from them originated the nerves; 2. That the nerves were first developed, and that by their subsequent junction and union in one mass they formed the central parts of the nervous system; and 3. That all parts of the nervous system were developed together. Malpighi and Meckel, from observations made on the chick *in ovo*, endeavoured to show that the first nervous chord which became visible was that which represented the spinal marrow. Rolando, however, thought it was the *medulla oblongata.* Ackerman, however, denied this, and described the great sympathetic nerve, but especially the cardiac plexus, as the first portion of the nervous system which became visible. Serres held that the spinal chord and brain were developed after both nerves and that distinct ganglia could be traced in various organs; and referred, in proof of this, to acephalous fœtuses, whose nerves are fully developed though the brain be wanting, and to the circumstance more than once remarked, that if the eye be not formed, there is either no optic nerve, or only a trace. Desmoulins admits with M. Serres that the nerves do not originate from the brain or spinal chord, but he also denies that the one class of organs exists before the other. As to the arguments drawn from the occurrence of acephalous fœtuses, he agrees with Morgagni, Brunner, and Lallemand, that these cases do not prove an original non-development, but that these central parts have been subsequently destroyed in consequence of serous effusion, or other disease. The same arguments apply to the want of an optic nerve, where the eye is not developed. According to the time which has elapsed since the organ was destroyed, the optic nerve is found either rudimentary, as in Rudolphi's case, or completely wanting, as in Klinkosch's case, where, at the same time, the brain was only partially developed. Baer and Burdoch seem to agree that the view which harmonizes best with all recorded observations, is that which considers the development of central and peripheral parts to go on simultaneously, and which holds that the one part does not originate from the other.

M. Longet appears to adopt the views of Serres, in so far at least as this, that "the peripheral system of nerves seems to be developed independent of all primitive relation with the central nervous system," and then he adds, as corroborative of his views, that "the first is already very apparent, when the second is scarcely visible."

All anatomists agree, with the exception of Gall and Spurzheim, that the white cerebral matter is found before the gray; and Gall's assertions on this point have been shown to be incorrect by every late anatomical investigator.

M. Longet draws the following conclusions from the above statement:—

"1. No known relation exists primitively in the fœtus between the cerebro-spinal axis and the nerves. 2. The peripheral nervous system is already very apparent when the central nervous system is scarcely visible. 3. In monsters, we find the first formed in the absence of the second. 4. The development of the nervous system seems to go on from the circumference to the centre. 5. None of the primitive points of the nervous system ought to be regarded as centres of irradiation. 6. The *nisus formativus* organizes each of the organs in its proper place; there is succession in their development; but the one cannot be the efflorescence of the other. 7. It appears that the gray substance does not exist before the white substance; the reverse appears to be the fact. 8. The physiological laws which regulate nutrition are opposed to that view which considers the gray matter of the nervous system as the nutrient organ of the white; one solid substance cannot nourish another solid. 9. Before assuming the forms which are proper to it, the cerebro-spinal axis of man presents, in its successive evolutions, nearly the different forms of the same organ in the vertebrated animals of different classes."—Vol. i. p. 22–23.

M. Longet, in thus adopting the conclusions of M. Serres, with some inconsiderable modifications, appears to have gone further than the facts warrant. We agree with him that none of the points of the nervous system ought to be regarded as the centres from which all the other parts originate; but neither is it requisite to consider that the great central masses are developed from the nerves, as M. Serres and he endeavour to prove is the case. In the germinal vesicle of all animals, parts which become the spinal chord appear as soon as traces of a blood-vessel, or of nerves around it; in fact, the development of these three structures seems to occur so simultaneously that it is quite vain to attempt to prove that one part appears before the other. M. Serres lays great stress on the circumstance of the distinctness of the nerves as compared with the softness, fluidity, and apparent structureless condition of the brain and spinal marrow in the germ. But this is not at all to be wondered at. The brain and spinal cord are of no use to the fœtus till it is born or is hatched, or, more strictly speaking, till quickening takes place; but its ganglionic system of nerves, which are the first to be discovered in a state of distinct development, are required from the moment when the blood-vessels begin to circulate a fluid. These ganglionic nerves, and especially, as Ackermann remarked, the cardiac plexus, are the first nerves which become distinctly visible; but even at this stage the traces of the spinal cord are quite distinct.

Were the views of M. Serres correct, that nerves are primarily developed in the organs, and afterwards extend to form the central parts, when the eye was wanting, we ought not only to find no optic nerve, but also no *tubercula quadrigemina.* In all the recorded cases, however, these organs were present, though the optic nerve existed simply as a band of condensed cellular tissue, or had even entirely disappeared. The reference to the optic nerve, however, is a most unfortunate one for M. Serres's theory, because no traces of the eye or optic nerve can be discovered long after the brain and spinal chord are visible; and when the eye does become visible, the optic filament appears at the same moment, all the way from the brain to the back of the eye. It never appears in the eye before it is in connection with the brain; it is never seen disjoined from or pushing an extremity towards the brain. That the optic nerve, then, should be wanting, or be in a simple rudimentary state when the eye is wanting, is nothing more than what we might expect. If anything destroy the organ while yet it is imperfectly developed, the optic nerve, which is developed with that organ, is arrested in its growth; and that nerve, which requires a microscope to discover when the arrestment of development probably took place, could never be expected to be discovered after the animal was born. Recorded pathological cases, in which vision of an eye has been destroyed, have fully proved that from that moment the optic nerve begins to shrivel, and becomes finally reduced to a cartilaginous chord, or to a fibrous, filamentous, or ligamentous band. Even in these cases, Andral states that "the optic thalami are very seldom altered;" and Magendie states that the atrophy rarely extends beyond the commissure, which could scarcely be the case if they owed their origin to, and depended on, the optic nerves.

The evidence derived from the consideration of acephalous monsters proves nothing more than that the nerves of organic life are independent of those of animal life, and the formation of both independent of the great nervous centres. Knowing, as we now do, that the ganglionic system of nerves regulates the involuntary acts and presides over the vascular system, and, of course, over secretion and nutrition,—knowing that the nerves of every organ are formed at the same moment as that organ itself—we can easily understand, that although, from some arrestment of development, the brain and spinal chord should cease to grow, or be destroyed, at a very early period of fœtal life, the other organs of the body, and the nerves of animal life of these organs, should be fully formed under the presiding action of the ganglionic system of nerves. But, supposing all this were the case, it leaves the question exactly where it was; or, if good for anything, it rather proves that the whole parts of the nervous system are dependent on the ganglionic system, which, in this case, ought to be the first developed. None of the phenomena of growth, however, seem to favour the views of M. Serres, that the nervous system is developed from its circumference to its centre. With regard to some organs of the fœtus, we know the very reverse is the case. Long after the brain and spinal chord are distinctly visible, the limbs begin to bud from the body; and, just in proportion to the growth of these limbs, are the nerves from the spinal chord seen to become more and more enlarged, and extend further and further into the limbs. Here, then, is a distinct case of the nerves being developed, as it were, from the spinal chord; but, as we explained above, we do not hold this to prove anything relative to the dependence of the development of the nerves from the chord, because, whether there had been a chord or none, the nutrient vessels, in continuing the growth of the body, would have furnished each organ with nerves, directed, as they ever are, by the ganglionic system; but these nerves of sensation and of voluntary motion are of no more use to the fœtus till its birth than are the brain and spinal chord. In fact, the brain, spinal chord, and nerves of animal life, are intended solely for extra-uterine life, so that they may be more or less completely arrested in their development, without the growth of the fœtus being materially interfered with. Those organs only in which the arrest of development takes place will be affected. Thus, if the arrest of development takes place in the brain, no cranium will be found, because there is no brain to protect. If it be in the spinal chord, no vertebral laminæ will cover the site of the defective organ. But this never will prove that these central organs are developed posterior to the nerves, unless we admit with Ackermann, that the ganglionic system of nerves are the first to make their appearance. Our own researches on this very point have led us to conclude that all parts of the nervous system appear so very nearly at the same time, that we have as yet failed to detect which was first developed. On this only we were able to satisfy ourselves, that the nerves in the neighbourhood of the blood-vessels, those which afterwards become the ganglionic or sympathetic system, were those which seemed first to become most perfectly organized.

M. Longet's second chapter is devoted to the consideration of the distinction of the organs of sensation from those of motion in the nervous system. This is a subject on which we have in former volumes entered somewhat largely, so that there appears less necessity for dilating on it here. M. Longet, however, has himself repeated many of the original experiments of Bell and Bellingeri, and, from using greater precautions than appears to have been done by the latter, and also by employing electricity or galvanism as the irritating agent, he satisfied himself that the views of Sir Charles Bell were those which were most correct, viz. that the posterior roots of the spinal nerves were alone concerned in sensation, and the anterior roots in those of motion. M. Longet saw nothing in his experiments which could in the least authenticate the views of Bellingeri, as to the nerves from the posterior column of the spinal chord being the antagonists of those from the anterior columns, nor did he ever meet with any symptom which indicated that the nerves from the posterior columns of the chord possessed the property of exciting muscular action. He, therefore, fully confirms the views of Sir Charles Bell, that every nerve originating from the posterior columns of the spinal chord, or from the prolongations of these chords into the brain, (restiform bodies,) are devoted to the conduction of sensations alone, and that all these have one character in common, viz. a ganglionic enlargement shortly after their origin from the nervous centres.

The author divides the nerves of sensation into two classes,—those of general and those of special sensation. The first includes the ganglionic portion of the trifacial, the glosso-pharyngeal, and the pneumo-gastric, together with the posterior roots of the thirty-one spinal nerves. The olfactory, optic, and auditory nerves form the latter class. M. Longet is far from believing that experiments on the brains of the lower animals will ever enable us to arrive at any conclusions relative to the central seat of motion or of sensation in man. Birds may have their cerebral lobes entirely removed, yet walk, fly, feel, and even live for months, as Flourens proved. The brain in them does not, therefore, appear to be the seat of motion or of sensation. Even in the rabbit the cerebral lobes and the *corpora striata* may be removed, and the animal retains the power of standing and walking, and, if it be pinched, it manifests pain by crying. Removal of the cerebellum only causes the movements to be executed disorderly, and the sensations, though diminished, are far from being entirely destroyed. In man, however, the smallest injury of the cerebral lobes will often cause general loss of both sensation and motion. We have, therefore, much to learn as to the minute anatomical peculiarities of the brain of these animals and of man before we can be expected either to account for the different effects of injuries of these parts, or in what manner their different parts are connected with the nerves of sensation or of voluntary motion.

The mode of action of sensitive and motor nerves forms the subject of the third chapter. That sensation may be perceived, or voluntary motion produced, it is necessary that the nerves be connected with the brain, either immediately or by means of the spinal marrow. When the nerves are divided, irritation of the free extremities of the first causes no appreciable effect, but irritation of those of the latter excites for a certain time muscular action. The nerves of sensation act only by conveying impressions to the brain, those of motion by transmitting the moving principle from the brain.

In experimenting on motor nerves it is constantly remarked that, after a nerve is cut across, no irritation of that part of the nerve which is still connected with the spinal marrow will excite contractions in muscles provided with nerves below the point injured. The primitive fibres of a plexus never inosculate with one another as do blood-vessels. Van Deen, Muller, and Kronenberg have fully proved that each of the primitive fibres remains isolated, communicates no nervous force to the others with which it associates, and simply contributes with them to form the enlarged nervous trunk which proceeds from the plexus. Thus dissection reveals that the three branches of spinal nerves which go to the thigh of the frog are distributed to different muscles. They form a plexus before being distributed to the thigh; but if the first branch be cut across, between the plexus and the spinal chord, all those parts supplied by nerves from it are deprived of the power of voluntary motion, which they would not have been had the primitive nerves in the plexus communicated with each other. Panizza, therefore, in asserting that there is inosculation of the primitive fibres at a plexus, and that injury of one of the branches of the nerve between the spinal chord and plexus does not arrest the power of volition in the branches of the nerve so cut, seems to have allowed himself to be deceived from not attending to the special distribution of each nerve; for Muller, Van Deen, and Longet, on repeating his own experiment, constantly observed the fact as above stated. M. Longet cites several analogous experiments he performed, with the view of ascertaining whether the plexus acted as it were the part of a new nervous centre, or allowed that free interchange of nervous force, that, though several branches going to that plexus were cut, the others which remained entire could convey the power of volition to all parts of the limb. He invariably found they could not. M. Longet very properly remarks, that simple observation might have led to the same conclusion,—that no inosculation can occur among the primitive fibres,—because we find the same nervous trunk supplying antagonist muscles. Thus, the upper and lower rectus of the eye are supplied by branches of the same nerve, and if any inosculation occurred at any part, the force of volition conveyed along the nerve would as often excite the wrong as the right muscle. The harmony and combination of movements, then, must originate in the brain itself, and be simply conducted by the several primitive nervous chords to those parts which we wish to move.

M. Longet next proceeds to point out the causes which excite or destroy the irritability of the motor nerves, the duration of this property when their connection with the nervous centres is cut off, and their influence on muscular irritability.

When various agents are applied to the cut free extremities of motor nerves, contractions are excited in the muscles to which they are distributed. Pinching and pricking, the application of galvanism, of potash, soda, ammonia, opium, chloride of barium, tartrate of antimony, chlorine, alcohol, &c. to the cut extremities have, in the hands of various experimenters, been seen to produce distinct muscular contractions. Heat and cold even produced contractions. M. Longet found, that applying the flame of a candle to the extremity of the nerve produced very violent contractions; but ice did not excite them so much. M. Longet also found that the local application of heat or of cold for a short time destroyed the irritability of the nerve at that place where these agents were applied. The same was produced by an aqueous solution of opium, of belladonna, &c.

It was reserved for M. Longet to determine exactly the influence of the nerves on muscular irritability. Legallois found that irritation of the sciatic nerve, after the spinal chord was destroyed, still excited muscular contractions. Muller, Sticker, Steinruck, Schön, and Günther, made more accurate experiments. They cut the sciatic nerves of various animals, and found that, after the lapse of many weeks and months, the excitation of the lower extremity of the cut nerve failed to excite muscular contractions. These experiments, however, neither proved the dependence of muscular irritability on the integrity of the nerves, nor the length of time when excitation of the cut extremity of the nerve ceased to excite muscular contractions. M. Longet proceeded differently. Instead of waiting for weeks or months before he tried whether irritation of its cut extremity would excite muscular contractions, he irritated it day by day, both by galvanism and mechanical agents, and constantly found that "its excitability was wholly lost after the fourth day." To ascertain this fact beyond a doubt, not only was the experiment frequently repeated, but some of the muscles to which branches of the cut nerve were distributed were exposed, and irritation of even these branches, by means of galvanic or mechanical agents, failed to excite the slightest muscular action. To render these results more striking the nerves on the opposite extremity were laid bare, when their slightest irritation produced the usual muscular contractions. These experiments thus demonstrate, says M. Longet, that we cannot admit that a principle analogous to what emanates from the cerebro-spinal axis is generated in the nervous chords; but, on the other hand, that it is necessary they remain attached to the spinal chord in order to remain excitable; and that their excitability is wholly destroyed within four complete days after their communication with the cerebro-spinal axis is cut off.

Seeing this was the case, it seemed of some consequence to ascertain how far the muscular irritability depended on nervous influence. Haller and his followers endeavoured to prove that the muscular contractility was independent of nervous influence, by tearing the heart from the body, or cutting a piece of muscle from the living animal. But in both of these cases the nerves were entire, and on being irritated could excite the muscular irritability. M. Longet, however, after he found that the excitation of the nerves which had been cut across ceased to convey the excitation to the muscles, found that the muscles themselves contracted freely on the slightest direct irritation, even for twelve weeks after the nerve lost its excitability.

When a nerve of sensation is cut across, irritation of its free extremity gives no pain, no sensation; but if the extremity still in communication with the cerebro-spinal axis be irritated, pain is instantly experienced. If the trunk of a sound nerve of sensation be injured, the pain is not only referred to the point injured, but also to the extreme branches of the nerve, as is experienced when the elbow is struck over the ulnar nerve. It is well known also that a man whose limb is amputated, refers the chief pain to the fingers or toes of the limb which is removed, and not to the cut extremity of the nerve on the face of the stump; and Muller has recorded many cases in which, even after the lapse of years, uneasy feelings, consequent on some irritation of the stump, have been referred to one part or other of the lost limb. Surgeons also are perfectly aware that section of a nerve frequently fails to remove neuralgia, because, if the seat of irritation be in the trunk of the nerve nearer the nervous centre than where it is cut across, the pain will still be referred to the same point, just as happened with the nerve on the face of the stump.

Having found that the nerves of motion lose all power of exciting muscles to contract four days after they are cut across, it became an object of some importance to ascertain how far the nerves of sensation influence muscular irritability. When M. Longet cut across a mixed nerve, as the sciatic, in a dog, voluntary motion and sensation were immediately extinguished in the limb. At the end of four days no irritation of the extremities of the nerve or of its branches caused muscular contractions, but at the end of fifteen days direct irritation of the muscle seemed to cause it to contract as actively as ever. It could still be excited to contract at the end of a month, but by seven weeks the contractions or irritation were scarcely perceptible. The muscular fibre was by this time much discoloured, and evidently degenerating; and, shortly after, the strongest direct stimulation could not excite the slightest contraction. The following experiment was also performed. The infra-orbital, the buccal, the anastomosing branches before the ear of the auriculo-temporal with the middle branch of the seventh pair of nerves were cut across, and portions removed to prevent reunion. All the filaments of the trifacial of one side were thus cut across which go to the muscles of the nostril and upper lip. Six weeks after the operation the muscles on that side were found pale and colourless, yet still irritable, but to a very inferior degree to those of the sound side of the face. The hairs had also fallen from the upper lip, which had a doughy feel, evidently due to defective nutrition. From this experiment M. Longet concludes that muscular irritability is to a certain extent dependent on the nerves of sensation, seeing that when the nerves of motion were alone cut across the muscular irritability remained tolerably entire for at least three months; whilst in this experiment, in which the nerves of sensation were alone cut across, the muscular irritability was notably diminished after a period of only six weeks. In reviewing his opinion, however, he modifies it so far as to allow that the whole may be accounted for by the organic changes which take place in the muscle, for, in all the instances, the muscular irritability was manifested so long as the muscle retained its proper physical properties, and only ceased when it became pale and degenerated.

The microscopic structure of nerves next engages M. Longet's attention, and after an examination of the varied statements of observers on this point, arrives at the conclusion that nerves, both of sensation and of motion, consist of cylindrical tubes filled with a matter which, in the recent nerve, is fluid, but speedily becomes granular. Modern research has not succeeded in proving the nature of this fluid, which has been variously supposed by microscopists to be "oleiform or mucilaginous." The nerves of sensation appear to consist of more delicate tubes than those of motion, as the least handling throws them into varicose dilatations. In their natural state, however, they are quite cylindrical. The nerves of the ganglionic system do not seem to differ in structure from those of the ordinary nerves, excepting always the gray ganglionic matter. The latest researches appear to show that the primitive fibres of the white matter of the brain and spinal chord are also composed of tubes. The gray matter of the brain, spinal chord, and ganglia, has been recognised to be composed of a granular substance, or of minute more or less globular-shaped bodies of various sizes. From this statement it would appear that the observations of Leuenhoeck and Malpighi, the fathers of the microscopic anatomy, however much they have been abused as having been made with imperfect instruments, come nearer the truth than modern microscopists will allow. Leuenhoeck described nerves as composed of parallel cylindrical tubes, on which he occasionally observed bulgings; and the brain as composed of straight parallel fibres or tubes, which had a particular distribution in reference to the surface of that organ. Malpighi also described nerves as composed of parallel cylindrical tubes, which he recognised to be filled with a fluid which he thought was of a serous nature; and modern microscopists, with all the aids of modern discovery and improved instruments, have not advanced one step farther. This fact is highly important; and shows that every one is not fitted for making such minute investigations. It requires the practice of a life to enable one to examine correctly. It requires one to begin with the simplest form of instruments and with low powers, and accustom himself to investigations with these, before he uses the more compound instruments and the higher powers. He requires also to be able to divest himself of all theories, otherwise he will never see minute objects but through that theory.

Modern research has done much towards ascertaining the mode in which nerves terminate. Rudolphi appears to have been the first who described them as terminating in a loop, which turned back on itself; and this fact has now been satisfactorily proved by the researches of Prevost and Dumas, Valentin, Emmert, Breschet, Burdach, Mandl, &c. In the case of the motor nerves, the last ramifications run parallel to each other, and generally also parallel with the muscular fibre, and, dividing into their primitive fibres, send these across the fibres of the muscle, when they either return to join the nervous branch they have left, or anastomose with a neighbouring one. The terminal loops of the nerve thus cross every muscular fibre. The nerves of sensation appear to terminate much in the same way, only that the loops are in general variously contorted before they return to reform a nervous twig by their reunion. In general, also, the primitive fibril does not return by the same branch from which it was given off, but joins an adjoining one. The mode in which the primitive fibres terminate in the brain has not yet been satisfactorily ascertained. Valentin asserts, they terminate in loops; but later observers, as Burdach and Longet, with the assistance of M. Mandl, have not been able to detect such a structure. Valentine must assuredly be wrong in his statement, that the primitive fibres of the nerves which join the spinal chord are continued onwards to the brain, inasmuch as the nerves of the axillary plexus alone would form a chord thicker than the spinal chord itself at any point from which its various branches were given off. What then becomes of all the nerves given off below this point? This is one of the points on which microscopic anatomists have not as yet thrown much light. Purely anatomical considerations would lead us to infer that the greater portion of the fibres originate from the spinal chord itself, and do not pass up through it to the brain. That there is an intimate connection between the brain and the nerves no one can deny, but when we see that the spinal chord is not materially diminished in size after giving off the large nerves, which, on each side of the body, form the axillary plexus,—nay, when we find that the spinal chord above the origins of these nerves is of the same diameter as it is below the origins of these nerves, but is sensibly enlarged where they are given off, we are almost infallibly led to the conclusion, that many, if not most of the spinal nerves originate in the chord itself, and are not continued onwards to the brain.

Before leaving the structure of the nerves, there is one point to which we think sufficient attention has not been paid, and that is, whether the nerves of sensation do not, at their terminal loops, unite with the nerves of motion, just in the same way as do the veins with the arteries. No inquirer seems to have taken up this interesting point; but there seems nothing improbable in the idea, that such is really the case. We know that the agency of volition, whatever that be, is transmitted in one direction only, viz. from the nervous centres to the terminal loop of the nerve; and we also know that sensations are transmitted solely in the opposite direction, viz. from the terminal loops towards the nervous centres. Seeing that this is the case, we are unable to discover the use of the ultimate fibrils of the nerve bending on themselves, and returning to the brain or spinal chord, unless it were that the *return* chord conveys sensation from the organ to the brain or chord, the *exit* chord conveys volition from the brain or spinal chord to the organ. The extreme difficulty, or rather impossibility, of tracing by the knife the minute subdivisions of the nerves, has hitherto prevented us from satisfying ourselves completely on this point, but we have already seen sufficient to show that it is most probably the mode in which these two sets of filaments are connected in every case. Our researches were confined chiefly to the nerves of the face, because there the nerves of motion are easily distinguished from those of sensation, and may be traced separately, almost to their final distribution. After tracing a minute branch of the fifth and of the seventh pairs of nerves to a particular muscle, and carrying the dissection as minutely as the unassisted eye could guide, we continued it by means of fine needles under the microscope, and on more than one occasion satisfied ourselves that the terminal loops of the nerves communicated by their one extremity with the sensatory portion of the fifth pair, and by their other with the motor portion of the seventh pair.

If our observations are correct on this subject, we ought to find that those nerves which preside over special sensations, as the olfactory, optic, and auditory nerves, do not terminate in loops, or bend back on themselves. Accordingly, we find that, excepting Valentin and Breschet, who assert that all nerves terminate in loops, neither Treviranus, Gottsche, Müller, Mandl, nor other recent observers, have been able to detect the termination by returning loops in these nerves of special sensation. We may add, that we have also failed to detect the presence of returning loops in these nerves. Reason, therefore, as well as our own observation, so far as it has yet gone, would seem to render it probable that the nerves of common sensation unite with those of motion in the same way as arteries do with veins; that is to say, that each ultimate filament of a motor nerve, originating from the *anterior* column of the spinal chord, becomes a nerve of sensation at its looped extremity, and, as such, returns to become united with the *posterior* column of the chord. Nerves of sensation, we know, are present in all parts of the body, but the microscope has hitherto only detected in muscles nervous loops disposed in one way, and the above explanation seems to be the only way of accounting for the fact of these organs being possessed of sensation as well as motion.

M. Longet, in his next chapter, details the chemical composition of the nervous substance, and, after quoting the observations of chemists on this point, regards the results of M. Fremy as those most entitled to credit. He found in the brain a white matter, which he terms cerebric acid, cholesterine, a particular fatty acid which he names the oleophosphoric, traces of oleine and margarine, the usual fatty acids, albumen, and sulphur. M. Fremy also found that considerable difference existed between the composition of the cortical and of the white matter of the brain. The fatty substances above named were also found in the white substance; only traces of them existed in the cortical or gray matter. In fact, he states, that when the fatty matters were all removed from the white matter of the brain, the residue presented the same chemical characters as the gray matter.

So little attention has been paid to the chemical analysis of the cerebellum, spinal marrow, ganglia, or nerves, that M. Longet states they would all require to be repeated before any conclusions could be drawn as to their similarity or dissimilarity in composition with the brain.

The consideration of the nervous agency forms the subject of the sixth chapter. In order to explain the phenomena of physical life in man and the animals, most writers admit the presence of an imponderable agent, which has been variously designated, nervous principle, agent, or fluid, nervous force, active principle of the nerves, &c. Baron Cuvier thought it very likely that nerves acted by means of some imponderable fluid; but physiologists have been in vain attempting to discover the nature of this fluid. Some consider it as identical with the electric fluid; others that it is only analogous to it, and may, like the magnetic fluid, be but a simple modification thereof; while a third class consider it to be an agent *sui generis*. The first two opinions, which, in fact, are but modifications of one another, are based on the following conclusions:—

1. Electricity can supply the nervous agency, in producing contraction of the muscles. 2. Wilson Philip caused food to be digested in the stomach of an animal whose pneumo-gastric nerves were cut, by passing a current of electricity through them. 3. Dutrochet has seen many muscular filaments produced in an emulsion of the white of eggs, by passing a current of electricity through it. 4. By placing needles in the nerves of man and of animals, Beclard, Beraudi, and Prevost have seen them become magnetic. 5. David, by means of nervous electricity, produced a deviation of the magnet in the multiplicator of Schweiger. 6. By means of the galvanometer of Peltier, M. Donné discovered in the living body electrical currents flowing from the skin to the mucous membranes, from the liver to the stomach, &c. 7. M. Lembert and Jobert having exposed the spinal column in living animals, saw small light threads attracted towards it. 8. Vassali-Eandi discovered free electricity in the blood, urine, and bile of different vertebrated animals, which, when concentrated by conductors, excited contractions in the muscles of the frog's thigh. 9. Nervous and electrical transmissions are equally rapid. 10. Electricity and nervous agency may be excited by heat, chemical action, frictions, percussions, &c. 11. Certain phenomena are equally produced by both, as elevation of temperature, expansion, decomposition of some substances, and the formation of others. 12. Electricity, when applied to the body, causes effects which seem exclusively regulated by nervous agency, as convulsions and muscular contractions. 13. *Lastly*, The identity of the two seems to be put beyond the power of dispute, by what occurs in the electrical fishes. According to some observers, the electrical shocks given by these animals are so dependent on innervation, that section or ligature of the nerves which go to the electrical apparatus, or removal of the posterior lobe of the brain, destroyed the electrical properties of the organs.

M. Longet then states that, even though the whole of the above conclusions were proved, they would not demonstrate the identity of the nervous agency with electricity, but only show they were analogous. He then, at great length, proceeds to examine each argument separately, and adduces under each head all the experiments, *pro* and *con*, which have been made by different experimenters. The result of this inquiry, in almost every case, is to show that the facts may be very differently explained, and that simple mechanical or chemical irritations are quite as efficacious in exciting the nervous agency as electricity itself. He shows, from the experiments of others, that all parts of the body give out electricity; but he has never succeeded in causing the slightest magnetic power in a needle, by inserting it either into a nerve, or into the brain or chord themselves. As to the electrical apparatus in fishes, he shows that M. Matteucci proved that the electricity in the torpedo is given off in one direction only, and that tying the nerve sufficed to prevent the animal giving shocks, which it could not have done had the agent been truly electrical. He therefore infers that the electricity in those fishes which generate it, is most probably formed in the organ itself. He then shows that M. Person ascertained, by direct experiments, that nerves were inferior conductors of electricity to metals; that they were no better conductors than muscular fibre or other moist membrane; that destruction of the pulp of the nerve, though it completely destroyed the conducting power of the nervous agency, did not in the slightest interfere with the conduction of electricity; and that an electric current passes freely from the nerve to the muscle or adjoining parts, taking always the direct line between the positive and negative wires.

The conclusions which M. Longet very justly draws from his minute investigation of all that has yet been written on the subject is,

"1. There does not exist at present any direct and certain proof in favour of the hypothesis of electrical currents in nerves.

"2. Electricity and the nervous force are not identical.

"3. In the present state of our knowledge it would be rashness to affirm that they are totally different; and that they do not offer the slightest analogy.

"4. The neurilema, which transmits even feeble electrical currents, cannot transmit the nervous agency.

"5. Electricity is probably only a simple excitant of the persistant nervous force, and its action is probably the same as those of mechanical or chemical irritants."—Vol. i. p. 143.

For farther information on this interesting subject, we beg to refer to a paper "On the Nature of the Nervous Agency," in the present Number, which takes very decided views of the matter.

Such, then, is a short abstract of the first part of M. Longet's work. The remainder is devoted to a very full detail of the anatomy and physiology of each individual portion of the nervous system. The anatomical details necessarily occupy by far the greater portion of this part of the work; but, under almost every head, after giving the special anatomy of the organ in question, the comparative anatomy of the same structure in the lower classes is also described, then the functions of the organ, and pathological facts. In this manner is described, not only the brain as a mass, but each individual part of it, as the *tuber annulare*, the pineal gland, the *corpora striata*, the *corpus callosum*, the optic thalami, &c. For the minute details of the anatomy and comparative anatomy of these individual parts of the nervous system, we must refer to M. Longet's work itself, merely noticing any new facts brought forward by the learned author, or any new light thrown by him on the physiology of the organs in question.

In describing the *dura mater*, M. Longet states that he has been able to authenticate the researches of Cruveilhier as to the existence of distinct nerves in its structure, a fact denied by many eminent anatomists. These nerves come exclusively from the trifacial nerve; 1. from the large branch before its fusion with the Gasserian ganglion; 2. from the ganglion itself; and 3. from the ophthalmic nerve. The filaments which appear to come from the sympathetic really proceed from the ophthalmic branch.

The author then demonstrates, by a reference to several experiments of Magendie's, most of which he has himself repeated, that there exists a fluid in the spinal canal of every living vertebrated animal. This fluid is always met with in man, and is found in the cellular tissue, between the *pia mater* and arachnoid, and communicates directly with the fluid of the fourth ventricle. Many facts are mentioned to show the directness of this communication. The probable uses of this fluid appears to be to regulate the amount of pressure on the centres of the nervous system. Both Magendie and M. Longet found that, during inspirations, this fluid descended from the ventricles to the spinal chord, but, during expiration, again ascended to the ventricles. This observation is accounted for on the supposition that, during expiration, the blood stagnates in the venous sinuses of the spinal canal, and by its pressure forces the fluid of the chord back upon the brain; but, during inspiration, the venous blood flows freely, or is drawn towards the heart, and thus leaves the fluid to descend from the brain to the chord.

Many interesting points are touched on in considering the spinal chord of the inferior animals. M. Longet justly limits the name spinal chord to that portion of the nervous matter which is contained within the spinal canal, as, if the bulb were also to be reckoned as belonging to it, most of the cerebral nerves must also be considered as spinal, seeing they originate from it or from its prolongations. It has been usual to attribute the want of a tail in man to the ascent of the spinal chord at a certain period of utero-gestation. The fact is undoubted; but M. Longet shows, from a reference to the comparative anatomy of the chord, that the conclusion is not legitimate. Birds, which of all animals have the shortest tails, have the spinal chord prolonged even to the coccygeal vertebræ; whereas, in many monkeys with prehensile tails, and in the kangaroo, whose length of tail is undoubted, the spinal chord stops at the lumbar region, and does not even occupy the sacrum. In many fishes, again, notably in the *Lophius piscatorius*, the spinal chord ends at the third vertebra, the nerves alone being continued to the end of that fish's long tail. In many of the lower animals, in birds especially, M. Serres showed that enlargements occur on the spinal chord, either on the point from which the nerves go to the anterior or to the posterior extremities. If the animal were one which chiefly made use of its anterior extremities, as the eagle, swallow, &c., M. Serres showed that the dilatation on the spinal chord at that point exceeded that from which the nerves to the posterior extremities originated. But if the animal were one which used chiefly its posterior extremities, as many of the wading birds, the cassowary, ostrich, &c., the posterior dilatation greatly exceeded the anterior in size. M. Desmoulins and M. Longet have shown that there are exceptions to this rule, but the general fact remains uncontradicted. Something of the same nature is observed in man. The spinal chord, where it gives off the first cervical nerves, is somewhat contracted; but where it gives off the large branches which go to form the brachial plexus, the dimensions of the chord are notably increased.

It is to M. Longet that physiology is chiefly indebted for having ascertained, beyond the possibility of dispute, the functions of the anterior and posterior columns of the spinal chord, and has thus definitively settled the question as to whether the views of Sir Charles Bell or of Bellingeri are the correct ones. The numerous experiments, which M. Longet performed for the establishment of this much disputed subject, were all repeated several times before a commission of the Institute of France, and received their entire approval. Sir Charles Bell, it may be recollected, attributed the regulation of the voluntary motions to those nerves which originate from the anterior column of the spinal chord, while the sensations of the body were directed by those nerves which had their origin in the posterior column of that organ. Bellingeri, on the other hand, while he maintained that sensation was alone communicated through the nerves which originated from the posterior column, attributed to these nerves a motive power also, by virtue of which they regulated the movements of the muscles concerned in extension, elevation, and abduction. To the nerves which were connected with the anterior columns, he attributed the regulation of those muscles only which were concerned in motions of flexion and adduction. According to his view, the anterior column of the chord formed the antagonist of the posterior chord, and it was only to those nerves which originated from the gray matter of the posterior chord that he attributed the power of conducting sensation.

The physiologists who have taken up this important subject since the papers of Sir Charles Bell and Bellingeri were published, have been far from being at one either as to the correctness of Sir Charles' views or of those of Bellingeri. Magendie, in his first experiments, published in 1823, espoused the views of Sir Charles Bell; but in his work on the Nervous System, published in 1839, admitted that the anterior column of the chord possessed "a very apparent sensibility." Schoeps, in 1823, and Rolando, in 1828, attributed the regulation of both sensation and motion equally to anterior and posterior chord; and Calmeil, in 1828, while he adopted the same views, was inclined, with Bellingeri, to attribute the conductors of sensation to the gray matter of the chord. Backer, in 1830, published a few very unsatisfactory experiments, from which he deduced that Sir Charles Bell's views were incorrect. Seubert, in 1833, tried in vain to deduce any conclusions from the results of numerous experiments he performed; and Müller, in 1840, in his work on the Physiology of the Nervous System, after detailing the results of numerous experiments, declares, that "the hypothesis which regards the anterior columns of the spinal chord as motors, and the posterior as sensitive, rests on no satisfactory facts, either experimental or pathological."

Such was the unsatisfactory state of the question when M. Longet, in 1840, began that brilliant course of experiments by which he to the full proved the correctness of the views of the British physiologist; and when we find among the names of those who witnessed his experiments, and were satisfied with their correctness, those of Flourens, Blainville, Breschet, Lallemand, Marshall Hall, Cruveilhier, Bouillaud, Blandin, Gerdy, &c., we may rest assured that their results were as conclusive and satisfactory as the nature of the subject would allow.

M. Longet's experiments were made on adult dogs, as he found that in them the size of the different columns of the chord enabled him to distinguish between them and isolate them far more satisfactorily than in young animals, and also that the irritation of one column by galvanism or mechanical agency had less tendency to be propagated by any reflex movement to the other parts of the chord. When he exposed the chord in the lumbar region, and after carefully opening its dura matral covering, cut it across, he found that no irritation of the free extremity of the posterior columns of the chord could excite muscular contractions; but when the extremities of the same columns, still in connection with the brain, were touched or irritated by galvanism, the animal experienced the most excruciating pain, as expressed by its cries. When the free extremities of the anterior columns were irritated, the posterior extremities, which were previously hanging paralysed, were thrown into strong convulsive contractions; but irritation of the extremities of the anterior columns still in connection with the brain, failed to excite any sensation, or any convulsive movement. These facts prove, adds M. Longet, that sensation is conveyed only from the nerves to the brain, and voluntary motion from the brain to the nerves. The same results followed when the spinal chord was exposed in the neck. When the chord was cut across, voluntary motion and sensation of the whole body were destroyed. When the free extremities of the anterior columns of the chord were irritated by galvanism, convulsions of the whole muscles of the body were occasioned, but no effect followed irritation of the free extremities of the posterior columns. Irritation of the extremities of the anterior columns, still in connection with the brain, failed to excite any sensation; that of the posterior columns produced excruciating pain. The anterior columns, then, of the spinal chord, and the nerves proceeding from them, preside over voluntary motion. The posterior columns of the chord, and the nerves in connection with them, preside over sensation.

As to the function of the lateral columns of the spinal chord, M. Longet has not been able to arrive at any determinate conclusions. That the nerves which originate from this portion of the chord go to those muscles which are concerned in the respiratory function, is undoubted; and section of the chord above their origin arrests the respiratory motions. He has never yet succeeded in cutting across the lateral columns of the chord without injuring the adjoining columns, so that no certain conclusions could be drawn from his experiments. He states, however, a very strong negative fact, which would tend to establish the views of Sir Charles Bell, viz. that when he cut the anterior columns of the spinal chord at the root of the neck, the respiratory motions continued as before, though the motion of all other parts supplied by nerves below the point of section was arrested. Even the neutral result of his experiments thus goes to prove the correctness of so much of Sir Charles Bell's views, as that the lateral columns of the spinal chord and the nerves proceeding from them preside, to a certain extent, over the motions of respiration. M. Longet, however, demonstrates that Sir Charles Bell allowed his theory to lead him into some assertions which will not bear examination. Thus Sir Charles stated that the pneumo-gastric and glosso-pharyngeal nerve originated from these lateral columns of the chord, and were consequently respiratory nerves, possessed neither of the powers of sensation nor of voluntary motion. It is now, however, fully ascertained that these very nerves originate from the sides of the restiform bodies, in the same groove from which all the nerves of sensation originate, and the distribution of these nerves to the mucous membrane of the larynx, trachea, bronchii, œsophagus, stomach, tongue, pharynx, &c., proves them to be so far, at least, sentient nerves. M. Longet, however, satisfactorily ascertained, that neither the lateral columns nor the nerves originating from them are possessed of sensation.

A little further on, when considering the physiology of the *medulla oblongata*, the author endeavours to prove, chiefly from the experiments of Flourens, that that organ specially presides over the respiratory function. But after a rather severe criticism of Sir Charles Bell's order of respiratory nerves, he is forced to admit that "the functions of the columns formerly described as the intermediate or lateral columns of the spinal chord, influence respiration." And he then adds, that the structure of these columns, and the bodies which are their continuations in the *medulla oblongata* (the olivary bodies) is such as to render them "fitted to represent a centre of innervation in the spinal chord;" for while the pyramidal and restiform bodies are exclusively composed of white fibres, a considerable quantity of yellowish-gray matter, abundantly supplied with arteries, is found in the centre of these parts. If we exclude, then, the pneumo-gastric and glosso-pharyngeal nerves, which ought never to have been included among the respiratory nerves, the original division of Sir Charles Bell will remain as a physiological truth.

Our limits will not permit us to enter into all the physiological points of interest discussed at length in M. Longet's work. After a full examination of all the experiments and arguments as to whether the spinal marrow influences the movements of the heart and of the circulation, the author states that no unanswerable arguments have yet been brought forward to prove that the spinal chord exerts no influence on the movements of the heart in the adult; while, on the other hand, numerous facts, founded on experiment and the phenomena of disease, have established the necessary connection of the spinal chord with the maintenance of the circulation. He in like manner shows that lesions of the chord affect the integrity of nutrition and secretion, and also, to a certain extent, of calorification.

M. Longet next enters at considerable length into the inquiry, whether the spinal chord is to be considered simply as an organ of conduction, like the nerves, or whether it forms a true centre of nervous energy. From a consideration of all its phenomena, he shows that it must be considered as a nervous centre, and that, in some cases of acephalous fœtuses, its nervous *centrality* is remarkable. It is to its property as a nervous centre he attributes the existence of that reflex function first described by Prochaska, demonstrated by Sir Charles Bell, and more lately taken up by Marshall Hall, by virtue of which, sensations propagated to the chord are reflected on the motor nerves. M. Longet is far, however, from agreeing with Marshall Hall's "pure hypothesis" of the existence of special excito-motory nerves, but is inclined, in so far as the phenomena connected with the spinal chord are concerned, to adopt the explanations of them given by Müller.

The pathological facts by which M. Longet establishes the fact of the anterior columns of the chord presiding over voluntary motion, and the posterior columns over sensation, are of extreme interest, and well deserve careful perusal. Cases are related where the anterior columns were more or less completely diseased, and in them all the parts supplied by nerves below the morbid structure were deprived of the power of motion, while sensation remained unimpaired. Where the lesion was confined to one side, that same side of the body was alone affected. Other cases are related where the posterior columns were alone found diseased, and in them sensation was destroyed, but the power of motion remained unimpaired. One very curious case is related in which only an obscure impression or sensation was conveyed to the brain, and in this case it was found that several white filaments had escaped the diseased action which involved the other parts of the posterior columns.

As to the functions of the *tuber annulare*, M. Longet states that it is not only a conducting organ, but also a centre of innervation. Irritation of the posterior part of the tuber causes acute pain, a fact easily explained by its anatomical relations, seeing that the restiform bodies, which are continuations of the posterior columns of the chord, send fibres to this part. Injury of the anterior part of the tuber causes no pain, but convulsive motions of the whole limbs and face. The *tuber annulare* is therefore one of the highest points of the brain, which, on being irritated, gives rise to muscular contractions; for neither the optic thalami, the *corpora striata*, the cerebrum, or cerebellum, exert any direct influence on muscular contractions. It is a remarkable fact, as showing the importance of this part of the nervous system, that in rabbits and young dogs, the hemispheres of the brain, the *corpora striata*, the optic thalami, the *tubercula quadrigemina*, and even the cerebellum, may be removed, and yet the animals live for upwards of an hour, their circulation and respiration going on regularly, and their common sensations so entire as to cause the animals to cry with pain if the tail is strongly pinched. If one of the hairs of its moustache be pulled, or ammonia be held to its nose, it makes an effort with its fore-feet to remove the source of irritation. If the tuber be removed, every function is abolished with the exception of respiration and circulation, which continue for a little while.

M. Longet confirms the results of Magendie's experiments as to injury of the peduncles of the cerebellum causing a rotatory motion of the animal. He shows, however, from a reference to his own and M. Lafargue's experiments, and also to a few pathological cases, that when one crus is injured the rotatory movement is in the opposite direction of that indicated by Magendie. Thus, if the right crus be cut across, the animal turns round from the right to the left, as, in consequence of the crossing of the fibres, the opposite side of the body is affected with the paralysis. The same was witnessed when the peduncles of the cerebrum were cut across. The animal ran round in a circle, having the injured side of the brain on the outer side of the circle, or, in other words, with the semi-paralysed side of the body towards the centre of the circle.

From a careful examination of fourteen cases of disease of the *tuber annulare* in man, cases which he records, M. Longet draws the conclusion that epileptic fits generally precede the paralysis of the trunk and limbs; that the patients always die of asphyxia; and that, when the lesion is situated on one side only, the opposite side of the body is affected, thus showing the decussation of the fibres.

The *tubercula quadrigemina* are shown to be continuous with the optic thalami anteriorly and with the olivary bodies behind, and that on their integrity depends to a great extent that of the sense of vision. The true nature of these bodies has been long misunderstood. Even in the lower animals they have been variously described as the cerebral lobes, optic thalami, *tubercula majora*, *tubercula olivaria*, &c. By a reference to the comparative anatomy of these organs, and to their development in the fœtus, M. Longet shows that they are continuous with the optic nerves, as had been demonstrated previously by Carus and Tiedemann. In all M. Longet's experiments removal of the *tubercula quadrigemina* caused total blindness in animals and birds; results which were perfectly in accordance with those of M. Flourens, but at variance with those of Magendie published in 1836. M. Magendie has since, however, admitted the influence of these bodies on the sense of vision. M. Longet found that the sense of vision remained after removal of the cerebral lobes. When this was done to some mammalia and birds, and the animals placed in a dark apartment, whenever a light was brought near the eyes, the iris contracted, often the eyelids winked; and the animal followed the motion of the light with its eyes. Irritation of the surface of the tubercula did not give rise to any expressions of pain in M. Longet's experiments, but when the interior was injured, the animal expressed by its cries the most acute suffering. Mechanical irritation of the surface, or even of the gray matter, never caused convulsive muscular contractions: in order to produce this effect it was necessary to continue the incision to the medullary fibres. A number of interesting cases are detailed in which, during life, a greater or lesser amount of blindness was observed, and after death the tubercula were found more or less diseased; convulsive, epileptic, or paralytic symptoms often attended along with the blindness, facts sufficiently accounted for by other morbid degenerations in other parts of the brain.

In noticing the physiological action of the optic thalami, M. Longet remarks, that they have far less influence on vision than the *tubercula quadrigemina*. As to their power of regulating the movements of the upper extremities, as was endeavoured to be proved by Foville and Serres, neither his own experiments, nor the numerous pathological cases of lesion of these lobes, recorded by Andral, seem to countenance such an idea. Andral relates the history of 75 cases in which the lesion was limited either to the *corpora striata* or to the optic thalami. In 40 of these the paralysis affected both the upper and lower extremities of one side, and of this number the lesion was limited in 21 cases to the *corpora striata*, and in the remaining 19 to the optic thalami. In 23 of the 75 cases the paralysis was limited to one of the upper extremities; in 11 of which the lesion was met with in the *corpora striata*, and in 10 in the optic lobes. In 12 of the 75 cases the paralysis was limited to the lower extremity; the lesion being found in 10 cases in the *corpora striata*, and in 2 in the optic lobes. From these facts M. Andral drew the conclusion, that, in the present state of our knowledge, we cannot assign in the brain distinct seats of movement to the upper and lower extremities, and M. Longet quite agrees with him. It may be added, that all experiments demonstrate that irritations, both of the *corpora striata* and optic thalami, neither excite sensations nor muscular contractions.

We must refer to the work itself for many interesting anatomical points connected with the pineal gland, *septum lucidum*, fornix, *corpora albicantia*, *tuber cinereum*, pituitary gland, ventricles, and cerebral lobes. Almost nothing is known as yet relative to the physiology of these parts, nor does M. Longet appear to have succeeded in throwing any additional light on this obscure subject. This only seems to be pretty generally agreed on, that the cerebral lobes preside over the intellectual and perceptive faculties and instincts. In idiots it is generally the cerebral lobes alone which are atrophied; all the other parts of the brain are in a natural condition, and have a normal development.

This fact, therefore, tends to prove that the higher faculties of the soul are intimately connected with the integrity of the cerebral lobes. Many anatomists, and among them Bichat, maintained that if the volume of the two hemispheres of the brain differed, there existed considerable intellectual inferiority, and such a defect or imparity of reasoning power as to prevent the individual from arriving at fixed conclusions relative to things. It is a remarkable fact, that Bichat's own brain proved the fallacy of this assumption,—for the one cerebral lobe was, after his death, found smaller than the other. M. Longet, however, narrates three instructive cases in which the intellectual faculties were perfect, and yet in which the cerebral lobes were found of very different dimensions. In the first case the right hemisphere of the brain was a half smaller than the left; in the second, the left hemisphere was at least a third less than the right; and in the third, the whole right hemisphere of the brain had disappeared, and its place been supplied by a watery fluid. M. Longet narrates the history of many cases of injury of the cerebral lobes, in which loss of substance took place, and yet the intellects remained entire. After a full review of all these cases, he arrives at the following conclusions:—

> "In man the hemispheres of the brain may be partially disorganized, or even undergo a notable loss of substance, without being followed by any appreciable affection of the intellectual powers.
>
> "The same negative result is observed when the disorganization or loss of substance takes place at the expense of the anterior or posterior lobes of the cerebral hemispheres.
>
> "The exercise of the sensorial function continues unimpaired notwithstanding such grave lesions.
>
> "On the other hand, the locomotive functions are more or less severely affected, and epileptic fits frequently ensue."—Vol. I. p. 679.

The learned author next endeavours, by a reference to pathological facts, to experiments on living animals, and to the comparative anatomy of the brain in the lower animals, to ascertain whether there is any truth in the phrenological doctrine of the localization of the intellectual and moral faculties in particular parts of the brain. After a very full examination of the question in all its bearings, he shows that, however specious many of the reasonings of the phrenologists may be, they are quite at variance with recorded pathological facts, with the results of direct experiment, and with the comparative anatomy of the brain and skulls of the lower animals.

In mentioning the weight of the cerebellum, M. Longet takes occasion to refer to the interesting researches of M. Leuret as to the influence of castration on the development of this organ. M.

Leuret, instead of trusting to his eye-sight alone to determine the relative bulk of the cerebellum, weighed both it and the brain of a number of stallions, mares, and castrated horses or geldings. Contrary to the assertions of Gall and all phrenologists, M. Leuret found that the size of the stallion's cerebellum was less than that of the mare's, and both of these less than that of the gelding's. Thus the average weight of the cerebellum in the mare and stallion was only 61 grammes, while in the gelding it amounted to 70 grammes. The cerebrum of the stallion, however, generally weighed more than that of the gelding, being on an average 488 grammes in the stallion, but only 419 grammes in the gelding. M. Flourens, on the other hand, found that removal of the cerebellum of a cock did not prevent that animal manifesting the usual sexual desires; and eight months after removal of the cerebellum the testicles were found of enormous size.

In a subsequent part of his work, M. Longet enters more fully into the examination of the phrenological doctrine of the influence of the cerebellum on the generative system, and proves that it is at variance with recorded pathological facts and with the comparative anatomy of that organ. M. Longet not only relates a very instructive case, in which a girl given to masturbation had no cerebellum, but he shows that where erection of the penis is observed during disease of the cerebellum, it is always dependent on some pressure or morbid alteration of the origin of the spinal chord, and that erection is a more common symptom in many injuries of the spinal chord than it is in disease or injury of the cerebellum. It is besides to be remembered, that Calmeil, Magendie, and Flourens found that removal of the cerebellum did not destroy the sexual desires in animals, but, on the other hand, they found that removal of the cerebral lobes produced this effect.

In all M. Longet's experiments, as in those of Flourens, Magendie, Bouillaud, Calmeil, and Hertwig, the cerebellum appeared insensible to ordinary irritations, nor did injury of its substance cause any convulsive muscular action. Even its removal did not extinguish the power of motion, but only produced a want of harmony in the movements of the animal. In man, however, injury or disease of the lobes of the cerebellum has been found to be attended in many cases with loss of motion of the opposite side of the body from that of the lobe affected, and M. Longet relates several such cases. M. Andral, however, who has collected no fewer than 93 cases of disease of the cerebellum, agrees that the want of harmony in the movements was the chief physiological fact which could be deduced from them. Pathology thus bears out experimental physiology in proving the cerebellum to be the organ which presides over the regulation or harmony of the voluntary movements. The author has collected a number of cases of diseased cerebellum in man, for the purpose of ascertaining whether the intellects are generally affected, or to what extent. The general result has been, that, even in cases where tumours or abscesses have compressed or materially destroyed the cerebellum, the intellects have been very little affected, and the sensations scarcely at all.

The movements of the brain next engage the author's attention; and as the subject is one which has in all ages been the object of inquiry to the physician, it is treated at considerable length. It is well known that when the soft fontanelles on an infant's head are examined, the brain is seen to be the subject of an alternate elevation and depression. The same is observed in the adult, if the bones of the cranium be so injured as to expose the brain or its membranes. The causes of this movement have given rise to much disputation, for while some maintained that it was due to the influence of respiration, others have attributed it to the arterial pulsations, while a third class have asserted that these two kinds of movement subsisted simultaneously. These, as well as the various mechanisms by which the movements were supposed to take place, are discussed at great length by M. Longet, and the conclusions which he arrives at, while they differ somewhat from those of most late writers on the nervous system, seem to be alone those to which reason and experiment lead. He shows, by a reference to incontrovertible experiments, that so long as the skull is entire and solid—so long as no atmospheric pressure is exerted on the soft surface of the brain, no distinct movement of elevation or depression is observed. The moment, however, atmospheric pressure is allowed to exert its force on the surface, distinct motions of elevation and depression are observed synchronous with the respiratory acts. In addition to this movement, however, distinct pulsatory movements are observed synchronous with the arterial beats. For the able review of the different theories on this subject we must refer to M. Longet's work; but his conclusions are as follows:—

" 1. The brain does not move in the adult so long as the skull is unbroken. It augments its mass during expiration; it diminishes its mass during inspiration, but its volume never varies.

2. It moves in infants so long as the sutures of the cranium are not united. It moves also when the parietes of the cranium of the adult have been destroyed for a greater or lesser extent by pathological causes, or by operations.

3. In all these cases the movement is due to the alternate turgescence and depletion of the vessels of the brain, and not to a locomotion of the organ. Locomotion of the brain is impossible.

4. These movements are of two kinds. It is easy to satisfy

ourselves in infants, that the one corresponds with the contractions of the heart, the other with the respiratory movements. These last are the most marked.

5. The turgescence or elevation of the brain corresponds with the expiration. It is produced by the stagnation of the venous blood in the encephalic veins, and by the greater afflux of arterial blood. The depression of the brain corresponds with the inspiration. It is produced by the afflux of the encephalic venous blood towards the thoracic organs, and by the concomitant lowering of the arterial circulation."—Vol. i. p. 790.

The influence of the circulation on the functions of the encephalon are next noticed. After referring to several cases in which death has followed the application of a ligature to even one of the carotid arteries, M. Longet shows that such is not the common result, but that, in very numerous instances, phenomena are produced which can alone be attributed to the derangement of the cerebral circulation. These are dimness or loss of vision in the eye of the side operated on; hemiplegia of the opposite side of the body; and very frequently notable impairment of the intellectual faculties. Syncope or convulsive fits of coughing have also been noticed the moment the ligature was tied.

In animals, as might be expected, such serious symptoms are not produced by ligature of one carotid artery. Even both carotid arteries have been tied in dogs without producing notable disorder in the cerebral functions. Ligature of the vertebral arteries in the lower animals gives rise to symptoms more nearly resembling those observed in man, when one or both carotids have been tied. In Sir Astley Cooper's experiments, when one vertebral artery was tied, dyspnœa was produced; and when the ligature was tightened on the second, the difficulty of breathing was much increased, and the anterior extremities became paralytic. The animal died on the seventh day after the operation. Sir Charles Bell found that ligature of the vertebral arteries of the rabbit, or even their compression, produced instant death; and in Flourens' experiments, though he could tie the two carotids and one vertebral with only the effect of producing coma and paralysis, yet instant death followed the ligature of the other vertebral artery. In dogs, however, in whom pretty free inosculations occur between the vertebral and carotid arteries, Sir Astley Cooper found that both sets of vessels might be tied, and yet the animal, after a while, recover. Thus in one dog, in which he tied the same day the two carotids and two vertebral arteries, coma, stupor, hemiplegia, and convulsive movements were produced; but within three days these symptoms began to abate, and the animal recovered. In another instance the vertebral arteries were tied eight days after the carotids; paralysis of the fore extremities was instantly produced, but the animal was able to run about the next day. In a third dog, however, in which the vertebral arteries were tied nine days after ligature of the carotid, instant death was the result.

The remainder of M. Longet's work is devoted to the anatomical examination of the various nerves of the body; but as mere anatomical details prove of little general interest, we must limit our further notice to some points of physiology connected with one or two of the cerebral nerves.

Finding fault with the division of cerebral nerves proposed by Sir Charles Bell, as inconsistent, in more cases than one, both with their anatomical characters and physiological uses, M. Longet divides the cerebral nerves into three orders; 1*st*, Nerves of special sensation, (olfactory, optic, and auditory); 2*d*, Nerves of general sensibility, (ganglionic portion of the trifacial, the glosso-pharyngeal, and pneumo-gastric); 3*d*, Nerves which preside at one and the same time over voluntary and respiratory motions, (the common motor nerve of the eye, the trochleator or pathetic nerve, the non-ganglionic portion of the trifacial, the external motor of the eye, the facial, the spinal, and the hypoglossal nerves.)

The experiments of M. Magendie, which M. Longet has repeated, satisfactorily prove that the nerves of special sensation transmit to the brain those sensations alone for which nature destines them. Thus the olfactory and auditory nerves may be cut or torn without causing any pain. Even the irritation of galvanism only causes flashes of light to be perceived by the optic nerves, and sensations of sound by the auditory nerves. We know absolutely nothing relative to the cause of this difference in the transmission of the sensations, for the microscope can detect no difference between the nerves of special and those of common sensation. We are, therefore, perfectly in the dark as to whether their aptitude to receive these different impressions resides in the nerves themselves, or in the parts of the brain with which they communicate. M. Longet seems inclined to think that both of these may be the case,—that they may have a different structure as well as a different origin from the other nerves of sensation.

In noticing the experiments of Magendie, who endeavoured to prove that the sense of smell is in part directed by the branches of the fifth pair of nerves, which go to the nose, M. Longet shows that that physiologist mistook common sensation for the special sense of smell. When the olfactory nerves were destroyed, the sense of smell was destroyed, but the animals still remained sensible to the impression produced by the vapours of ammonia, or of strong acetic acid. But these same agents produce the same action when applied to the conjunctiva of the eye. The action produced was, therefore, nothing more than an excitation of the common sensibility of the part which is regulated by the fifth pair of nerves. It is true that when the fifth pair of nerves is diseased the sense of smell is greatly impaired, because, adds M. Longet, the secretion of the nasal mucus is deranged, and on its quality depends to a great extent the perfection of the sense of smell. Many interesting pathological cases are related, illustrative of these views, and satisfactorily prove that the sense of smell depends on the integrity of the olfactory nerves, the general sensibility of the nose to that of the fifth pair.

In enumerating the functions of the fifth pair of nerves, M. Longet relates a very satisfactory experiment which he has many times performed, which demonstrates that the fifth pair presides over the general sensibility of the face and parts to which its ramifications extend. He, with great care, cut the fifth pair of nerves across within the cranium of a rabbit, and when this was done, so carefully as not to injure either the seventh pair or the brain itself, it produced exquisite pain, and was followed by instant loss of sensation of all the one side of the face, so that the eye of the animal could be cut out, its teeth pulled out, its face cut or burned, without its exhibiting the slightest sensation of pain. In several experiments in which this operation has been attempted by others, the brain, or seventh pair of nerves, have been injured, hence the discordant results at which they arrived. In all M. Longet's experiments the movement remained unimpaired so long as the lesion was confined to the fifth pair of nerves.

Section of the fifth pair of nerves leads to rapid disorganization of the eye, and consequent loss of vision. Even within twenty-four hours after the section, the cornea has commenced to become opaque; within the second day the conjunctival covering is inflamed, and is secreting puriform matter; in about eight days the cornea becomes detached from the sclerotic coat by ulceration, after which the humours become turbid, more or less opaque, are discharged, and the eye shrinks. M. Longet states, "vision is never abolished, but as a consequence of the opacity of the cornea," but that even from the first it appears to be somewhat weakened. A very curious fact, noticed, but not explained, by Magendie, is, that when the fifth pair of nerves is cut across in the temporal fossa at the level of the semilunar ganglion, the nutrition of the eye is rapidly impaired in the way above mentioned; but if the section be made within the cranium near its origin, and before it traverses the petrous portion of the temporal bone, such an effect is very partially produced. M. Longet explains this phenomenon by attributing the disorganization, in the one case, to the section or injury of the semilunar ganglion, and the branches of the sympathetic, parts which are not hurt when the section is made within the cranium; and this opinion is supported by the fact, that when section is made of the cervical portion of the great sympathetic nerve of one side, the nutrition of the corresponding eye is immediately affected, and the pupil is contracted, as after section of the fifth pair beyond the semilunar ganglion.

In noticing the influence of the fifth pair of nerves on the sense of taste, M. Longet shows that section of the branch of the fifth pair, which goes to the tongue, destroys the tactile sensibility as well as the perception of taste over all that portion of the tongue to which the nerve is distributed. When the lingual branch of the fifth nerve was cut, the anterior portion of the tongue could be torn, cut, or burned without the animal exhibiting any sensation of pain. This nerve, however, only supplies the anterior two-thirds of the tongue, the posterior portion of that organ having tactile sensibility and its perception of taste guided by the glosso-pharyngeal nerve. The organs of taste have thus no special nerve for the manifestation of the sense of taste. This subject M. Longet examines at some length, and satisfactorily proves that the exclusive views of MM. Panizza and Magendie, the first of whom attributed the sense of taste to the glosso-pharyngeal alone, the other to the lingual branch of the fifth nerve alone, are not correct; but that the sense of taste and the general sensibility are at once supplied by the two nerves, the one supplying the back part of the tongue, the other the anterior and middle portions of that organ.

The author then reviews what has been at various times advanced, regarding the possibility of the fifth pair of nerves supplying the place of the special nerves of smell, of vision, and of hearing. After a very full examination of the subject, he arrives at the conclusion that it cannot, but that its integrity is required for the free development of these various senses, inasmuch as it regulates the secretions and nutrition of the organs which are fitted for the manifestation of these senses.

The non-ganglionic portion of the fifth pair of nerves is shown to have nothing to do with the respiratory function, as Sir Charles Bell maintained, nor yet to be distributed to the buccinator or orbicularis oris muscles. It is distributed to the temporal, external and internal pterygoid, mylo-hyoid and other muscles which raise, depress, and move laterally the lower jaw, and act on the soft palate. This distribution, therefore, while it disproves the assertions of Sir Charles Bell, also satisfactorily demonstrates the fallacy of Bellingeri's views of the antagonism of the nerves originating from different parts of the spinal columns, inasmuch as this one branch supplies at once both elevators and depressors of the lower jaw,—supplies muscles which are antagonists to one another.

A number of interesting pathological cases are recorded, in which the trifacial nerve was diseased, and the symptoms which were observed during life confirm, in almost every particular, the conclusions at which M. Longet has arrived relative to the functions of this nerve.

The glosso-pharyngeal nerve is proved to be simply a nerve of sensation, from its point of origin in the continuation of the posterior grove of the spinal chord, from its ganglionic character, and from the results of direct experiment. M. Longet, however, demonstrates that, in consequence of inosculations with the facial nerve, and pharyngeal branch of the spinal accessory nerve, it also exhibits, below its ganglionic portion, some phenomena of a nerve of motion. The glosso-pharyngeal nerve, along with the lingual branch of the fifth pair, presides over the sense of taste. M. Longet's experiments on this subject are the only ones which have satisfied us on this point. These of Dr Alcock and Dr John Reid were not free from objection. After section of this nerve on both sides, they introduced bitter substances into the mouth of the animal and found them to be instantly rejected. And how, indeed, could it be otherwise? The whole anterior three-fourths of the tongue, receiving its gustatory twigs from the trifacial nerve, transmitted the bitter taste to the sensorium quite independent of the cut nerves. M. Longet, however, after section of the nerves, applied the bitter substance to the back of the tongue,—to that part alone supplied by nerves from the glosso-pharyngeal trunk,—and found the animal quite insensible to the presence of the bitter drug. If the smallest portion, however, by accident, touched the anterior portion of the tongue, the animal instantly manifested by its motions the presence of the bitter substance, as had always occurred in Dr Alcock's and Dr John Reid's experiments. When the bitter substance was applied to the back of the tongue before the nerves were cut, the sensation of bitterness was instantly perceived.

At the same time that M. Longet thus satisfactorily demonstrates the influence of the glosso-pharyngeal nerve on the sense of taste, he shows how former experimentalists, as Magendie and Alcock, have allowed themselves to be deceived in attributing difficulty of swallowing to section of this nerve. From a reference, indeed, to M. Magendie's own experiments, and the physiological effects following the section, he renders it next to certain that that physiologist had cut across the pharyngeal branches of the spinal accessory nerves instead of the glosso-pharyngeal nerves. In all M. Longet's experiments, provided the section were limited to the glosso-pharyngeal alone, the general and special sensibility of the back part of the tongue and palate were alone affected; but if, from the animal resisting strongly, some of the pharyngeal branches of the spinal accessory nerves were injured, deglutition was instantly rendered very difficult. The slight contractions remarked in the pharyngeal muscles, when the glosso-pharyngeal nerve is irritated in the throat, is shown to be dependent on the presence of a few motor filaments which are united with that nerve, and not to any supposed reflex action, as some have supposed.

It is demonstrated that the pneumo-gastric and spinal accessory nerves, which form but one trunk when they leave the skull, stand, in their origins, in the relation of posterior and anterior roots of spinal nerves, the pneumo-gastric roots being, from their origin and functions, nerves of sensation, the spinal accessory nerves of motion. M. Longet, both by a copious reference to the experiments of others and to his own researches, establishes the double function of most of the branches of this compound nerve. He at the same time shows that, while some branches are chiefly composed of sensitive filaments, others are more especially formed of motor filaments. The many interesting points connected with the functions of this pair of nerves, such as their influence over the motions of the larynx, on the voice, on respiration, on the arterialization and fluidity of the blood, on the heart, œsophagus, stomach, &c., are reviewed with great ability. He shows how some physiologists have allowed themselves to be deceived as to the section of the recurrent nerves not completely abolishing the powers of voice. In adult animals it invariably abolishes all power of voice; but in young animals, from the peculiar conformation of the larynx, a sharp cry may still be elicited, from the rapid passage of the air over the passive *chordæ vocales*. Section of the recurrent nerves, which M. Longet shows supplies movement to the constrictors as well as the dilators of the glottis, contrary to the opinion of Magendie, does not necessarily cause speedy death by suffocation in adult dogs, but it does so in the young. This is accounted for by the peculiarities of the larynx in all young animals, which is then margined as it were by membranes, and scarcely supported by the still soft cartilages. When the recurrents, therefore, are cut, the whole parts are paralysed, the glottis is closed by the rapid rush of air to the lungs, and asphyxia is the consequence. In the adult animal, however, the parts are sufficiently rigid to resist, to a certain extent, the atmospheric pressure when the chest is dilated; in fact, M. Longet has kept dogs alive for five weeks after section of the recurrent nerves, and, when killed after this time, their lungs were found quite permeable, and exempt from all traces of engorgement.

The pneumo-gastric nerves, besides regulating the general sensibility of the trachea, bronchia, and lungs, are shown to influence the respiratory phenomena, in so far as the contraction and dilatation of the pulmonary tissue is concerned, and the proper arterialization and fluidity of the blood. Inasmuch as the respiratory efforts continue after section of these nerves, M. Longet attributes to the influence of the *medulla oblongata* the sensation of the necessity for respiration,—a sensation which is at once abolished by injury of that organ. The opinion of Professor Mayer of Bonn, that the fibrinous clots found in the heart and blood-vessels after death were attributable to the loss of influence of the pneumo-gastric nerves, has been often disputed. M. Longet having ascertained that dogs in whom these nerves were cut lived a certain time, and constantly presented these fibrinous clots in their heart and blood-vessels after death, cut these nerves in a number of dogs, and killed them at various periods after the section, and instantly examined them. After twenty-four hours, there was no pulmonary engorgement, and the blood was very fluid. After thirty-six hours, black, very soft clots, resembling black currant jelly, were found in the auricles and ventricles of the heart, and in the trunks of the pulmonary artery and aorta. Pulmonary engorgement had also begun in certain points. On the third, but especially on the fourth day, the pulmonary engorgement was very marked, and solid, yellowish coloured clots were found filling the auricles and ventricles, and insinuating themselves between the *columnæ carneæ*; they also extended to the pulmonary arteries and veins, even to their ramifications. These experiments were performed on thirty dogs. After section of the pneumo-gastric nerves, no dog lived longer than five days; many died on the second and third days. Rabbits never lived beyond thirty-six hours after section of these nerves.

As to the effect of section of the pneumo-gastric nerves on the pulsations of the heart, M. Longet remarks, that, after the section, the pulsations were invariably increased in frequency. In place of 60 or 70 per minute, they averaged 150. The pulsations, however, are irregular and incomplete; but, as the blood which reaches the heart is somewhat imperfectly arterialized, he is not satisfied that this frequency and irregularity of the pulsations ought to be attributed to want of nervous power alone. He is, however, inclined to believe that it is by means of the cardiac branches of the pneumo-gastric nerves that mental emotions affect the pulsations of the heart.

The contradictory results at which physiologists arrived relative to the influence of the pneumo-gastric nerve on the stomach, induced M. Longet to perform a number of experiments for the purpose of ascertaining whether irritation of that nerve excited muscular contractions of that organ. He found that, when the stomach was empty, no contractions were excited on irritating the pneumo-gastric nerve in the neck, by mechanical or electrical agents; but that, when the stomach was filled with food, such irritation not only excited contractions, but occasionally produced the hour-glass contraction to such an extent that it seemed as if strangulated at its middle by means of a cord. This contraction caused the more fluid aliments to pass out by the pyloric orifice. These results sufficiently account for the varied statements of different physiologists as to the influence of this nerve over the muscular action of the stomach; for while some hold that its irritation produces no sensible effect on this organ, others assert it always does. M. Longet concludes from his experiments, that section of the pneumo-gastric nerve does not prevent the sensations of hunger and thirst being felt, nor yet food from being digested. He simply found that the gastric juice was secreted in lesser quantity, and that the digestion was rendered extremely slow. As the experiments of Spallanzani and Beaumont have proved that simple rubbing the internal surface of the stomach causes a secretion of gastric fluid, and as section of the pneumo-gastric nerves prevents the usual rubbing action of the coats of the stomach against the food, M. Longet accounts in this way for the diminished quantity of fluid secreted, without supposing that this secretion is otherwise under the influence of these nerves. This diminished supply of gastric fluid, and the total absence of all the natural movements of the stomach on the food, may, he thinks, to a great extent, account for the very slow, yet, after all, complete digestion of the food in the stomach of the animal whose pneumo-gastric nerves have been divided. Section of these nerves did not prevent poisons being absorbed from the stomach, only they took a few minutes longer to produce their usual toxicological effect than in animals whose pneumo-gastrics were whole.

A number of curious facts are recorded relative to the organic changes produced on the lungs by section of the pneumo-gastric nerve of one side only. Dogs on whom this operation was performed all survived. Emphysema of the corresponding lung was almost invariably remarked in all those which were killed the third day after the section. On the seventh day the emphysema was less, but many points of the lung were engorged with blood. By the end of the second week the lung could still be inflated; but the engorgement was general, and rendered it less permeable to the air than the lung of the opposite side. At the end of six weeks only a few lobules here and there could be inflated; the rest was impermeable both to air and to the circulation of blood. The animals all got lean after section of one nerve. In one, which was observed for three and a half months after the operation, the voice was hoarse, the breathing was as if asthmatic under exertion, and a kind of whistling proceeded from the larynx; and vomiting occurred daily for the first month, but gradually disappeared afterwards.

The motor nerves which proceed from the brain are most minutely described. These are the third, fourth, and sixth pairs of nerves which go to the muscles of the eye; the facial, or *portio dura* of the seventh pair, which supplies with motor power all the muscles of the face; and the ninth pair, or hypo-glossal nerve, which is distributed to the muscles of the tongue. It is shown, from a reference to direct experiment, and to the effect of disease, that these are all solely nerves of motion.

The remainder of the work is devoted to the great sympathetic or ganglionic system of nerves; but it appears to be unnecessary to enter into M. Longet's views on this subject, seeing that they closely agree with those of M. Brachet, whose work was so fully noticed in our 45th and 46th volumes. Instead, therefore, of detailing his experiments and remarks on the views of other physiologists, we shall content ourselves with giving his general conclusions.

" 1. The great sympathetic undoubtedly exhibits a certain amount of sensibility to our ordinary means of irritation.

" 2. It is easy to demonstrate experimentally the motive action of the great sympathetic.

" 3. The ganglionic substance, which forms part of this nerve, but especially the gray matter of the spinal marrow, must be regarded as the sources of the activity of this nerve.

" 4. Facts are far from confirming the opinion which regards each sympathetic ganglion as a small centre which acts independently of all cerebro-spinal influence. If we cannot deny to the ganglionic swellings an active co-operation as centres of innervation, we are, at least, forced to allow that their own action is insufficient for the proper maintenance of the functions of the great sympathetic.

" 5. Every sudden impression on the central organs of the cerebro-spinal system reacts powerfully on the sympathetic system; and reciprocally acute impressions emanating from the organs to which the sympathetics are distributed, cause an intense reaction on the cerebro-spinal nervous system.

" 6. The existence of a reflex power in the sympathetic system, analogous to that of the spinal chord, is a hypothesis which experiment disproves.

" 7. If we are not generally sensible of impressions made on the organs to which the sympathetics are distributed,—if the will has no command over their movements, we must refer the cause of these peculiarities not to the ganglia themselves, as do Johnston and Reil, but to the circumstance, that these impressions are quenched in the spinal chord, and that the original fibres of the great sympathetic do not extend, like those of the ordinary nerves, to the source of the voluntary influence.

" 8. Seeing that the parts which act involuntarily, as the heart and intestinal canal, preserve, for some time after separation from the body, the type of their peristaltic and rhythmical movements, it follows that these are independent of the cerebro-spinal influence; and seeing that these movements continue even after the removal of the ganglia and ganglionic plexuses, they cannot be considered as under the guidance of that system. A similar phenomenon may depend on this, that, in the terminal ramifications of the sympathetic system, the nervous principle may dissipate itself in successive and periodic discharges.

" 9. If we irritate a cerebro-spinal nerve of motion by chemical, mechanical, or galvanic agencies, we excite contractions, which instantly follow the application of the irritant, and as instantaneously disappear. When the same irritants are applied to a branch of the sympathetic system, a few seconds always elapse before the motor reaction is observed; and it does not reach its maximum of intensity till after the irritating cause is removed. Thus, in the one case, contraction commences and ceases with the irritation; in the other, it only commences after the irritation, and endures for a longer period than it.

" 10. The accessory nerve of Willis, or spinal accessory, may be regarded as establishing a transition between the cerebral nerves and the great sympathetics; for, on the one hand, it animates muscles of animal life, (muscles of the larynx, &c.), and muscles of organic life, (the contractile tissues of the bronchii, œsophagus, stomach, and heart); and, on the other hand, it presents a mode of origin similar to the sympathetics. If the sympathetic originates, in fact, from the whole length of the spinal chord, by means of an infinity of roots, the spinal accessory arises from no inconsiderable portion of the same axis.

" 11. Since the works of Muller and Marshall Hall on the reflex properties of the brain and spinal marrow, we are not at liberty to explain many of the sympathetic reactions, as we were wont to do, by means of the ganglionic nervous system.

" 12. It seems probable, in proceeding by way of exclusion and analogy, that arterialization of the blood, and the secretion of bronchial mucus, are chiefly under the guidance of the sympathetic system.

" 13. Irritation of the cervical ganglia or of the cardiac sympathetic nerves renews the contractions of the heart after they have ceased.

" 14. The results of experiment and of the anatomy of the organ prevent us from admitting, with M. Brachet, that the cardiac ganglion is the source whence the heart derives its motive principle.

" 15. The sympathetic system has nothing to do with the contractions of the œsophagus; it only influences its mucous secretion.

" 16. The contractions of the stomach are not under the influence of the sympathetics, as Muller asserts; our own experiments demonstrate that they are entirely under that of the eighth pair of nerves. The secretion of the gastric juice has appeared to be distinctly influenced by the sympathetics; nevertheless, it is impossible to deny some direct influence from the eighth pair of nerves on this important secretion.

" 17. Chemical irritants and galvanism applied to the sympathetic system excite contractions in the small intestines after they have naturally ceased; we cannot, therefore, conclude, as M. Brachet has done, that these nerves have nothing to do with the motion of these tubes, but are simply confined to the direction of the intestinal absorption, exhalation, and secretion.

"18. The bladder of a living animal, exposed to the air, comports itself absolutely as the intestine,—that is to say, it becomes the seat of a vermicular motion, which, when extinct, may be revived by irritation of the sympathetic nerves. I am inclined to believe that, as in the intestinal tube, the sympathetic has two functions to perform: 1. to guide the involuntary contractions of the bladder; and, 2. to direct the mucous secretion; while the cerebro-spinal nerves which go to the bladder are simply for the purpose of making known the desire to micturate, and producing voluntary contraction of the neck of that organ.

"19. After irritation of the inferior lumbar and superior sacral portions of the sympathetic, very distinct contractions are observed, according to Valentin, in the *vesiculæ seminales* of the guinea-pig. These contractions, which are really peristaltic, begin at the posterior extremity of the vesiculæ, and are so strong when the animal is at the rutting season, that the contained fluid escapes by the urethral orifice.

"20. The uterus of a gravid female, when exposed to the air, executes peristaltic movements quite similar to those of the intestines, and this organ can discharge its contents after death. When the uterine movements have ceased they may be renewed by irritating the inferior lumbar and superior sacral portions of the sympathetics. When we consider that the uterus receives alone filaments from the sympathetics, that it contracts in the same manner as all other muscles under the influence of the ganglionic system, that the will has no power over its movements, we may well be surprised that M. Brachet has asserted that its movements, during parturition, are under the sole influence of the cerebro-spinal system.

"21. Very recently Muller and Remak have described a special class of fibres, which they term gray or organized fibres, as occurring in the cerebro-spinal and sympathetic nerves, independent of those of motion and of sensation, and these they suppose preside over the organic acts of secretion and nutrition. It is not irrational to conclude that they also come from the cerebro-spinal axis, and conduct the principle of their action both into this central axis, into the ganglia of the sympathetics, and into the ganglia of the cerebro-spinal system. This mode of viewing the subject would explain, on the one hand, the incontestable influence of the cerebro-spinal axis on secretion and nutrition, and, on the other, (by the medium of the ganglia multiplying the nervous force,) the persistence of organic acts in parts deprived of motion and sensation.

"22. If, from the facts and experiments which we have recorded, we are forced to attribute distinctive characters to the sympathetic system, we yet cannot but acknowledge that it possesses numerous analogies with the cerebro-spinal system. It is not correct to endeavour to prove them to have absolutely distinct functions from the cerebro-spinal system, by asserting that they preside over the nutritive and secretory functions, seeing that this latter system, as we have proved, also exercises the same function.

"23. The sympathetic system is a chain or reunion of cerebro-spinal nerves, destined for the use of the vegetative organs, which forms a nervous system fitted to maintain the conflict between animal and vegetative life. The numerous links of this chain are indicated by so many ganglia, which, besides, have their analogies in the cerebro-spinal nerves.

"24. The difference of properties which each exhibits is probably owing to an organic cause; in fact, we regard the cerebro-spinal system as continuous with the white matter of the spinal chord,—the sympathetics as especially connected with the gray matter of the same chord."—Vol. II. p. 629 to 634.

This able work is terminated by giving a short sketch of the ganglionic system in the Invertebrata. From the experiments and dissections of Newport, Muller, Grant, Geoffroy St Hilaire, &c., and the results of his own researches, M. Longet demonstrates that the ganglionic system of nerves in the invertebrated animals is not the analogue of the sympathetic system of the higher classes. On the contrary, their nervous axis, with the nerves issuing from it, represent at one and the same time the cerebro-spinal axis and nerves, as well as the sympathetic system, and even in many of these animals the sympathetic system is quite distinct from the principal chain of nervous ganglia. He also satisfactorily demonstrates by experiments that the cephalic ganglia of these inferior classes, but especially of the Articulata, in so far as their direction of sensation and movement are concerned, perform the same functions as the brain in the higher classes, and ought to be considered as its analogue.

ART. II.—*On Dysmenorrhœa and other Uterine Affections in connection with derangement of the Assimilating Functions.* By EDWARD RIGBY, M. D., &c. 8vo, Pp. 138.

IN laying before our readers Dr Rigby's work on dysmenorrhœa, it is our intention merely to give a concise review of the subject treated. We adopt this arrangement not only with the desire to do justice to the author, and at the same time to render our criticism intelligible to those who may not yet have perused the book for themselves, but likewise with the view of doing justice to readers. We think it due, however, to Dr Rigby to premise, that although he has advanced some new views on uterine pathology, he does not leave us in ignorance of the premise from which his conclusions are drawn, nor does he pretend to more originality than is consistent with the most scrupulous desire to award to others their due share of merit. He commences his book by an exposition of the present state of knowledge on the physiology of assimilation, in which are succinctly explained those great series of chemical changes to which our attention has been directed by Prout, Liebig, and others, and from the derangements of which Dr Rigby attempts to show both functional and organic disease of the generative and other organs frequently result.

In these introductory observations, which occupy the first part of the book, the author frequently appeals to Dr Prout in confirmation of his views, which we may concisely state to be as follows. He infers from various facts, that in rheumatic gouty habits certain uterine derangements often exist, which he has found intimately connected with the above-mentioned diathesis, and also with a general anormal condition of the system—in his own words, "dysmenorrhœa in a rheumatic or gouty habit." Thus, in those affections severally, are observed the secretion of lithic acid, urea, and lactic acid by the skin, whilst muco-albuminous matter is eliminated from the mucous system, exhibiting many modifications of disease, both of the alimentary canal, and likewise of the female generative organs. He then proceeds to show that such states are found to co-exist with an altered action of the function of the kidneys, in which the albuminous principle of the blood is, as it were, subjected to a natural alembick, by which its constituents become resolved into lithic acid, and lithate of ammonia, &c.—in fact, into the component elements of albumen and gelatine, and are discharged as excrementitious. At other times the renal organs exhibit these principles unchanged in the forms of serous or albuminous urine. Thus, in a word, the skin and mucous membranes are shown to be the great emunctories by which albumen and gelatine, when mal-assimilated or in excess, are discharged from the system. In this process various morbid affections are engendered in those organs, by which such phenomena are effected. In the enumeration of some of these results, Dr Rigby points out the remarkable fact, that the mucous membranes in different parts of the body, not merely of the stomach and intestines, but also of the uterus, vagina, and bladder, are capable of secreting gas under certain circumstances of mal-assimilation, which are evidently closely connected with rheumatic and gouty diathesis. This is accompanied by some interesting remarks on the formation and discharge of fibrinous exudations in dysmenorrhœal patients, together with the co-existence of local inflammatory disease of contiguous organs. Thus, the *os uteri*, kidneys, and ovaries are frequently implicated in these maladies. The various phases assumed by this distressing affection are clearly and practically detailed, whilst the author's favourite remedies, together with the cases in which their efficacy is advocated, evince a knowledge of therapeutics,—a branch of science too frequently neglected by the modern practitioner of medicine. Tabular views of urinary analyses are appended to these cases, in which are shown the frequent excess of urea, as well as of the lithic deposits in the progress of these affections. Dr Rigby observes, (p. 44,) "One very remarkable feature which I have observed in a large number of cases, has been the very considerable excess of urea, not occurring as an incidental and occasional phenomenon, but with a great degree of uniformity in those cases whose urine I have had constant and regular opportunities of examining. In every one of the analyses just referred to, urea was present in excess, and in most of them it was very considerable." The author concludes his work by offering a few concise rules, intelligible to all, for examining urine, and which will be found of great value to such members of the profession as may not have time or opportunity to consult the more voluminous authors on the subject of animal chemistry.

Our limits necessarily preclude more than a brief survey of the subject which Dr Rigby has treated; but we do not hesitate to recommend the book earnestly to the attentive consideration of the profession, from the conviction that, under a very unassuming garb, it contains much valuable matter, which is not only presented in a pleasing form, but which likewise promises to yield, in the hands of an intelligent practitioner, the means of facilitating the treatment of a class of diseases, which too often baffle our utmost efforts to relieve.

This work is further entitled to attention, as one proceeding from a physician whose opportunities for professional information have been much beyond common, and who has availed himself of these opportunities with great industry. If, therefore, the facts collected appear singular, and the views new and different from those usually promulgated, they ought to be received and examined with that degree of caution, which all facts and inferences, founded on careful observation, deserve. To use the language of his preface, his observations have neither been hastily nor recently made; they have been gradually collected for several years; and the author having enjoyed the privilege, since 1831, of attending the uterine cases among the out-patients at two of the great Hospitals of the metropolis, viz, St Thomas's and St Bartholomew's, we need hardly say his field for observation has been considerable. Dr Rigby also fills the office of physician to the hospital for the diseases of women, which has lately been established in London,—an institution which stands alone as exclusively appropriated to the reception of female ailments, and which, we are informed, yields such extensive opportunities of observation as nowhere else exist.

ART. III.—*A Bill for the better Regulation of Medical Practice throughout the United Kingdom.*

FOR the space of rather more than a quarter of a century a notion has been more or less firmly and extensively gaining ground, that the profession of medicine was requiring some degree of reform. This notion originated not with those external to the profession, but owed its birth principally to the observation and reflection of various members engaged in its practice, and well acquainted with all those peculiar circumstances which the exercise of such a profession is calculated to bring to light.

But what is meant, let us ask, in the first place, by reform in medicine? We have had reformation in religion, in doctrine, in church polity and in practice. We have had reform in parliament and in representation; and reform in the municipal constitution of our cities, towns, and boroughs. And if to these we have not attached clear and distinct ideas, it is not for want of practical illustrations and much experience. But to reform in medicine, what idea do we attach? As to reform in the doctrines and practice of the art, that is evidently an object not to be attained by legislation. Such change as is here implied is the result of the gradual increase of scientific knowledge, and the improvement of the practical departments of chemistry, pharmacy, therapeutics, and surgery. What, then, is meant by medical reform? The term thus employed manifestly refers to the social existence of medicine as a practical art or profession, and may be defined to mean the removal of all real grievances, abuses, and unnecessary impediments, the amendment of imperfections, the rectification of errors, and the general improvement of medicine and surgery in their actual applications, as means of diminishing the natural evils incident to the human race. Let us consider how the necessity of reform thus defined has become obvious.

It has been a matter of general observation, in the first place, that though the profession presented, as was admitted long ago by Pope, Addison, Swift, and Johnson, many individuals of great learning, much intelligence, and remarkable skill in the application of the practical resources of their art, yet often was it dishonoured by having to number among its professors persons not only of no learning and knowledge, but those who, from deficiency in moral principle and integrity, pursued the art as a trade, and scrupled not to have recourse to the most degrading arts to procure patients, and to maintain their influence over the sickly in body and the feeble in mind. It was also observed, that, as in other professions, while talent, learning, and integrity were often associated with a degree of modesty and dignity of mind, which prevented them from being appreciated by the world, ignorance and incapacity were fortified with such an amount of effrontery and assurance, and, not unfrequently, low cunning, that it must have implied a totally different constitution of society, and a different constitution of the human mind from what is observed to take place in the present state of affairs, to imagine that the former should be preferred to the latter. Multiplied professional errors were known from time to time to be committed, to the injury of patients and the discredit of the art; yet were there no efficient means of counteracting these errors, or of preventing their authors from continuing their pernicious career; and those who were aware of these evils were obliged to lament that which they had no power of amending.

Another set of grievances which were much and loudly complained of, were felt principally in the following manner. Persons with good professional education, well informed in all the branches of medical and surgical knowledge, placed themselves in situations in which they might render their knowledge useful to their fellow-creatures. No sooner did they this, however,—no sooner did they undertake to treat the disorders and accidents of the locality in which they were placed, than they were opposed and impeded by various rules, customs, or regulations, by which they learnt that they were by no means free agents in the exercise of the profession which they had chosen, and in preparing themselves for which, much time and labour had been occupied, and much money had been spent. The pretexts and reasons for the application of this system of annoyance and persecution were various. Sometimes the pretext assigned was, that the person or persons had been educated at a particular school; sometimes that he had been licensed by a particular board; sometimes that he possessed a degree; in other instances that he had no degree; in some instances he had not complied with the conditions prescribed by the incorporation of the place, and consequently had rendered himself liable to pains and penalties, fines, or even imprisonment for non-compliance, or it might be, for inability to comply.

No physician, however well qualified, could approach within seven miles' distance of London in any direction, to visit a patient or treat a disease, without the license of the College of Physicians of that city; and if he did so, he rendered himself liable to fine or even imprisonment at the instance of that body. The history of the Royal College in the reigns of Elizabeth, James I., and Charles I. and II., is a series of prosecutions, sometimes of quacks, old women, urine doctors, gardeners, and other interlopers, but occasionally of well-educated and well-qualified practitioners who had encroached on the well-protected preserves of the College, and who were thus made to feel the full weight of College indignation. In the thirty-fifth year of the reign of Queen

Elizabeth, Lord Treasurer Burleigh made application to the College for permission to be extended to Dr Butler, a professor of physic in the University of Cambridge, who sometimes had occasion to repair to London on private business, or at the request of persons anxious to have his advice, to give this advice when requested. The College answered that he should have the liberty of practice, if he would submit to the customary examinations of the College, and pay the usual fees due on that account. The members of the College of Surgeons even were not allowed to prescribe or dispense medicines, or treat any kind of internal disease without the risk of prosecution, and subsequent punishment in the shape of fine or imprisonment. (See cases of Jenkins and Read, 1602.) In the reign of James I. and Charles I. appear numerous prosecutions of surgeons, members of the College, and apothecaries, for undertaking the cure of internal diseases; and no one could prescribe an emetic or a purge without requesting the aid of a member of the College.

But not only were restrictions imposed on private practice. These exclusive privileges were applied forcibly and successfully in excluding individuals, otherwise well qualified, from holding public medical and other professional offices. In the distribution of the offices of physicians to hospitals, lunatic asylums, charity workhouses, parochial institutions, and similar establishments, the College, at least in London, have always exerted their right of preference with such energy and to such an extent, as to amount to an exclusion of all not fellows of that incorporation.

It is true that, by obtaining the license of the College, these impediments were occasionally removed and obviated. But even in the mode in which this was done the sovereign power of the Royal College was certainly exhibited in a manner offensive and degrading to the candidates for that honour, and by no means creditable to the College itself. To the circumstances of this we may afterwards advert. At present all that it is necessary to say is, that this mode of admitting licentiates, and the ignominious distinction attempted by the College to be fixed upon them, has for more than half a century been productive of the most bitter animosities between the College and the great mass of London physicians, with feelings of jealousy, envy, and all uncharitableness, and have indeed made the College be regarded with eyes of hatred and dislike, instead of affection and reverence, which would, under other circumstances, have been the tribute paid to that exalted body.

In the humbler walks of the profession grievances not less serious were felt. We have so often expatiated on the exclusive powers conferred on the apothecaries by their act, that it must be quite as disagreeable to our readers as it is, we assure them, to ourselves, to recur to that subject. We have therefore only to say, that, while we admit that the course of education prescribed by that body has been highly beneficial in improving the qualifications of the general practitioners, the part of their act authorizing them to prosecute and exclude others not members of their society, has proved most oppressive to individuals and injurious to the interests of the public. Even the last attempt to engraft the powers of the worshipful society upon those of the Poor Law Commissioners, can be viewed in no other light than the monopoly of a barbarous age, and the impolitic developement of an ignorant and self-willed despotism. On this, however, it is unnecessary to dwell, as we took occasion recently to bring its merits fully before the public.

The first and main cause of all these evils, and of many others which were daily coming to notice, was believed to be imperfect education and want of uniformity in the qualifications required to constitute a medical practitioner. To a certain extent this idea was not without some foundation. A very large proportion of those to whom the public would most readily have recourse for assistance in sickness, and in the case of injuries and accidents, were, in truth, persons either imperfectly educated, or without systematic education at all, and who had picked up, as is vulgarly said, any information which they possessed in a rambling, desultory manner, in whatever situations they were placed, and from whatever sources they had access to. Physicians, doubtless, there were in all the large towns, who had received education, both literary and professional, at universities and hospitals; and various places could boast of surgeons who had walked, as it was styled, the metropolitan hospitals, and perhaps had done a little more than walked those great and useful receptacles of sickness and injury. But these distinguished and well-qualified persons were not known to the great masses, who required their aid; or, if known, they were shunned as being expensive attendants; and the result was, that the majority of the middling, and all the lower ranks, applied to more humble but totally unqualified practitioners.

The inequality of education, and the consequent inequality of qualification, was indeed very great; and this want of uniformity was, from the particular circumstances in which the study of medicine was placed, and the situations in which it was pursued, rendered more remarkable than it could otherwise be with regard to any other learned profession. Physicians received, or ought to have received, a full university education, securing thereby the advantages of classical literature, mathematical instruction, and considerable information in the physical sciences; and, besides this, they were understood to have spent several years in the acquisition of anatomical, physiological, and medical knowledge at the best medical schools, and the largest and best conducted hospitals. Surgeons, also, who aspired at the first ranks in their profession, had, in general, received the elements of a good liberal education, in some instances at one of the collegiate or free schools, and had spent one or two years at one of the universities, and, besides, had studied anatomy practically for several seasons, and surgery at the most eligible hospitals and under the most able teachers.

But from the standards now referred to there were numerous deviations. While the graduates educated at the English universities have enjoyed the advantages of very full, and often accurate, instruction in literature and science, and several of those educated upon similar principles at the Scottish universities were also persons whose attainments in literature and science were perfectly adequate to sustain the characters of well-informed physicians; many, it must be acknowledged, appeared with the degree of doctor in medicine, whose learning and information, as well as their professional knowledge, were certainly not adequate to support that honoured name. Among those also trained as surgeons, it too often happened that the preliminary education was imperfect and scanty, while, probably, the professional attainments in anatomical and surgical knowledge were highly respectable; or it might be that the latter were defective as well as the former.

While such was the state of professional information and attainment among those who occupied the first ranks in the professions of medicine and surgery, it was unreasonable to expect that anything like uniformity could distinguish those of the more humble rank. In point of fact there was nothing uniform, except in the meagreness of information and the frequency of incompetency in knowledge. Many persons destined for the situation of country practitioners had the title of surgeon without any of the qualifications, except a very scanty knowledge of anatomy, certainly the most important and indispensable, but still not the only needful information, and an amount of modest assurance plentiful in the inverse ratio of their professional knowledge. In the days, indeed, to which we allude, all medical and surgical knowledge was imagined to consist in the study of anatomy for one or two winters, and a desultory attendance on an hospital or hospitals for one, or at most two seasons. A crude and imperfect system of physiology, taught at snatches, with anatomy,—a little of the surgery of dislocations, fractures, and other injuries,—no systematic pathology,—no general views of therapeutics,—were all that went to form the future surgeon and general practitioner. Many practitioners educated on this meagre and inefficient standard, are still flourishing in large and lucrative occupation.

Many persons, probably by far the majority, undertook the responsible duties of medical practitioner with even more limited education and qualifications than the scale now specified implies. A single course of lectures on anatomy, with such chemistry and pharmacy as an apothecary's shop affords, and a few months' attendance on an hospital, formed all the professional education which too often fell to the lot of those destined to act in provincial and country situations, as medical and surgical practitioner, and man-midwife. In such situations, that is, in all but the metropolis and a few county and cathedral towns, the medical profession presented an almost uniform dead-flat of ignorance and imperfect information. In such situations medicine and surgery could scarcely be called liberal arts. There was no inducement to make them such. Their professors had little interest, except that arising from the consciousness of the necessity of more perfect information,—of seeking for more knowledge; and the result too often was, that, in the struggle for employment, subsistence, and wealth, recourse was had to arts which must have been despised by the ingenuous, and condemned by the upright and honourable.

These results were pretty much the direct consequence of the multiplicity of boards and the low standard required by them to obtain their license. All the better class of medical practitioners in England and Wales applied to the College of Surgeons of London for their diploma; and the license from this body was certainly better than no qualification, with which, too often, country practitioners were satisfied. But the Royal College in those days required merely attendance on two or three courses of anatomy, on one of chemistry, and attendance at one of the recognised hospitals. The curriculum of education actually prescribed was as meagre as it could be, and left a great deal to be filled up at the discretion of the candidate. It must not be denied, however, that the scanty education thus enjoined was in many instances afterwards enlarged, rectified, and extended by the diligence and industry of individuals, by the exertion of their own powers of observation and reflection, and by the necessary cultivation which real talents undergo, when placed in circumstances calculated to call them forth.

This was, however, leaving too much to accidental circumstances, and expecting a great deal too much from human wisdom and judgment. Hence the inequality in education continued as great as ever.

Many medical practitioners, in the meanwhile, educated certainly upon a more comprehensive, and, in some respects, a more uniform plan, as to professional instruction, were from time to time ushered into the great field of public employment. The small College of Edinburgh, the youngest of all the British universities, had, at an early period, possessed one or two medical lectureships; one on botany, in 1676; and one on chemistry, in 1713; and to these, from the circumstance chiefly of an Infirmary being early established in that city, and another peculiar circumstance in the constitution of that establishment, additions were from time to time made, until, before the close of the eighteenth century, attendance on seven separate courses of lectures was pronounced necessary to render any one capable of applying for a degree as doctor and practiser of medicine. These were, 1. Anatomy and Surgery; 2. Chemistry and Chemical Pharmacy; 3. Institutions or Theory of Medicine, embracing Physiology, Pathology, and Therapeutics; 4. Materia Medica; 5. Practice of Medicine; 6. Botany; and 7. Clinical Medicine. At the same time, the College of Surgeons of the same place required of candidates, for their diploma, attendance on the same course, excepting Botany and Clinical Medicine, but adding Midwifery, with the Diseases of Women and Children. As of the numbers who, at that time, resorted to Edinburgh College or University, as it has since been denominated, the great majority came, not to obtain the degree, so much as to procure good and efficient education in medicine and surgery, and to practise the same, either with the diploma of the Edinburgh College of Surgeons, or that of the London College of Surgeons; a considerable number of well-informed practitioners were from time to time distributed over England, and sent to supply the exigencies of the public service. The passion for the possession of the medical degree had not yet become general. The education, however, of many, as conducted at this school, was, with the exception of Botany and Clinical Medicine, the latter certainly most important, nearly as good as that for the degree; and as the large proportion of students bred at the Edinburgh school aspired only to the title of surgeon, this was one means of insuring a reasonable amount of professional instruction in those destined to act as general practitioners.

Here, however, it is necessary to advert to the other circumstance in the constitution of the School of Medicine at the College of Edinburgh, which tended to make it be so much resorted to by those who were desirous to obtain useful and efficient information in medicine and surgery. This circumstance was the obligation required by the two great English Universities, of the pupils of all their Colleges and Halls to subscribe the thirty-nine articles of the Church of England, and the complete exemption from any religious test in the College of Edinburgh.

Previous to the year 1720, the medical establishment in the College of Edinburgh was not greater than that in Oxford or in Cambridge; but various causes concurred to enlarge it with considerable rapidity.

A lectureship on Botany had existed since 1676; but it was merely nominal, as there is no proof that lectures were delivered. A lectureship on Chemistry was founded in 1713, the first incumbent being at once professor of Hebrew and professor of Chemistry. And a sort of nominal lectureship on Medicine was held, first by Sir Robert Sibbald, and then by Dr Pitcairn. At length, in 1705, the lectureship on Anatomy was created by the appointment of Mr Robert Elliot, surgeon-apothecary, to that office. The vacancy caused by his death in 1714 was followed by the appointment of Mr Adam Drummond, who, however, appears to have performed the duties of his office chiefly by the aid of his assistant, Mr John Macgill. The retirement of these gentlemen, in January 1720, made way for the appointment of Mr Alexander Monro, who may be regarded as the first actually teaching Professor of Anatomy in the then small College of Edinburgh. In 1724 a professorship of the Institutes and Practice of Medicine was created, and Dr Porterfield appointed to that duty, It does not appear, however, that he actually taught; for in 1726 we find that Drs Andrew St Clair and John Rutherford were appointed Professors of the Theory and Practice of Medicine, and Drs Andrew Plummer and John Innes were appointed Professors of Chemistry. Soon after the Theory and Practice of Medicine were made separate charges, by the former being taught by Dr St Clair.

In London at this period lectures were delivered by such men as Cowper, Cheselden, Douglas, Nichols, and Sharp; and that was the great resort of the medical students of England. They would have resorted to the universities more numerously; but subscription was a complete barrier. Many were Dissenters and members of the Society of Friends, or otherwise held about subscription, conscientious scruples, which even genuine sons of the Church have been known to entertain. All persons, therefore, labouring under this peculiar conscientious feeling, and who, nevertheless, wished to obtain the benefits of education within a university, and any university honours, were thus precluded from doing so; and they had only two alternatives. The one was to go to Utrecht or Leyden, or some similar continental university, where the same rules were not enforced, or to go to Edinburgh, which, of course, was equally lax. To the latter even it had not become as yet fashionable to resort, and all those who aspired at the rank and title of graduate, generally studied at one or other of the continental universities. From the greater facility and convenience, however, in repairing to Scotland, and certainly from the benefits of clinical instruction at the hospital, which were now (1747) beginning to become known, pupils became more numerous. This also reacted in leading to the enlargement of the school. The business of teaching Botany and Materia Medica was separated into two offices; a separate lectureship for the Institutions or Theory of Medicine was established; and the whole system was rendered more efficient.

Of the advantages of this open and liberal, or it may be, lax system, the English universities had completely deprived themselves. By the severity of their enactments regarding the admission of pupils, they shut their doors against all Dissenters, all members of the Society of Friends, and all persons too conscientious to comply with subscription. The result was, that they had only that small proportion of pupils who did not hesitate to subscribe. This afforded no encouragement either to teach regularly, or to enlarge their medical school; and hence they were obliged to allow their own pupils, who were eventually to receive their degree, to obtain their medical knowledge wherever it could most easily and perfectly be obtained.

From this system of exclusion resulted good as well as evil. The two English universities actually but indirectly contributed to the rise of the Medical School in the small and comparatively recently established College of Edinburgh. They compelled numbers of young Englishmen, annually increasing, to resort thither for the benefits of medical education in a university; they crowded the small and incommodious class-rooms till they could hold no more; and they filled the purses of the professors with much good current coin of the realm, not to mention the streams of money which were constantly running into the hands of lodging-house keepers and tradesmen, all of whom thus shared unconsciously in the beneficial results of a penal enactment of two monastic establishments. The English universities acted in the most disinterested manner. By conscientious adherence to their statutes, they not only sent out of the country annually a considerable sum of money, but they contributed, certainly without being aware of it, to the exaltation of a rival institution, and one which probably they were led at first to despise from its poverty and insignificance.

Whether all this was prudent or not, on the part of the universities, it is not for us to inquire. Perhaps it might not occur to the dignitaries of the universities, as it would do to people taking sound views of such matters, that if medical students, or students of any kind, would not subscribe the thirty-nine articles, they rather required university instruction than to be excluded from it; that, had they been admitted without subscription, their views might gradually have been enlarged, and the severity of their scruples have become relaxed, so that eventually they might have seen the propriety of subscribing; and that, under all circumstances, if they had obstinately refused to subscribe, it was better to bring up such recusants under the maternal wings of the orthodox universities, and give them as much as they wished of human learning in a good orthodox way, than suffer them to run wild in a northern college, whose teachers cared for none of those things, and most of whose doctrines were supposed to be tinged with the cold and calculating metaphysics of Hume and his followers. Perhaps it did not occur to the persons having the charge of this important ceremonial, that men might be perfectly religious, and pious, and good Christians, according to the New Testament, though they did not and could not subscribe these articles; and that subscription to a formulary of this kind, constructed by fallible human beings, might prove the compliance, assentation, and subserviency of the parties subscribing, but could never demonstrate either their piety or their sincerity.

Such, however, was the result of this practice. It has been commonly supposed that the Edinburgh School owed its crowds of students, at the end of last century and the beginning of the present, to the celebrity of its teachers alone. That its teachers were, at that time, men both of talent and industry, it would be idle to deny. But that this was not the sole cause clearly results from the facts now specified, and still more from the analogous fact, that, since the English students have been provided with the means of obtaining a good university education, unembarrassed by the severities and restrictions imposed by religious distinctions, the numbers resorting to Edinburgh from England have greatly diminished. Several of the lecturers in that establishment are certainly not less able and not less distinguished than their predecessors; all the divisions of medical and surgical science are better taught. Yet the numbers of students are diminished and diminishing; and there seems to be no prospect of their soon again arising to their former standard. The truth is, that the numbers of medical pupils resorting to Edinburgh, towards the close of the eighteenth and the beginning of the nineteenth century, were, by particular causes, pushed far beyond what would have been their natural amount, had matters been allowed to pursue their own course; and now, when the causes referred to are operating with less force, the numbers are returning to the rate, at which probably, but for the disturbing forces referred to, they would have naturally and spontaneously stood.

We have referred to some beneficial effects arising from the enforcement of these statutes. Evils proceeded from them also. Not the smallest was the mode of the operation of this academical statute in the College of Physicians of London. That body refused to admit to their fellowship, or in any way to recognize any graduates but those of Oxford or Cambridge, or Trinity College, Dublin; and, as none could obtain the degree from these universities without previous subscription, a religious test thus became the means of admitting to, or excluding from whatever privileges pertained to Fellows of the College of Physicians. Though this was not quite equivalent to excluding a man from practising his profession in London, or from holding the office of physician to any of the great metropolitan hospitals, yet it was in actual operation equivalent; and if any one, not holding an Oxford or

Cambridge degree, ever was appointed to such offices, it was rare, and owing to some peculiar circumstance or circumstances, either of great and overpowering talents, or the interest of powerful friends, by whom he had been enabled to triumph over this formidable opposition. Numerous instances certainly occurred of men of great professional attainments, high character, and much experience, being totally prevented from rendering their services available to the public, in consequence of the operation of this test. Then followed various other evils which cannot well be mentioned in this place, since they fall more properly under another head. This enactment, in short, of the English universities has been, though unintentionally, productive of good, yet ultimately most pregnant with serious evils.

From this long digression we must now return to the view which we were taking of the progressive improvement of medical education in various parts of the kingdom.

The course of education prescribed by the College of Surgeons in Edinburgh was, soon after the commencement of the present century, enlarged and much improved by the addition of separate substantive courses of lectures on Surgery and Clinical Surgery. At this time, at no school in Great Britain was so complete provision made for the education of medical students, and by no board was so full and complete a course of education required.

These arrangements, however, served only to render the inequality of education and qualification the greater.

In 1815, as is well known, the English apothecaries applied for, and obtained permission, to regulate and superintend the education of the general practitioners in England and Wales; and by their subsequent arrangements and provisions, they have certainly improved greatly the education of this class of practitioners. Their course of education, however, is still much below that prescribed by the College of Surgeons of Edinburgh, which has been greatly enlarged and improved during the last twenty years.

There is still, indeed, immense diversity between the courses required by the different licensing boards, which are susceptible of comparison. Thus, the courses prescribed respectively by the Colleges of Surgeons of London, Dublin, and Edinburgh, differ widely from each other, though it is impossible to doubt that a candidate educated so as to be admitted to examination, and merit the license of either of these bodies, ought to be educated in the same manner, and instructed in the same branches of medical and surgical knowledge. The courses prescribed by the Society of Apothecaries in London and that in Dublin also differ from each other, and differ more from the courses prescribed by other of the surgical colleges, though it must be allowed that persons in the position of apothecaries, which now means general practitioner and man-midwife require as full and comprehensive an education as those who are to practise as surgeons. In England, indeed, for the last fifteen or twenty years, the inconveniences resulting from this disparity have been practically obviated by medical men destined for country practice, having at once the license or membership of the London College, and the license of the worshipful Company of Apothecaries. But the disparity continues not the less.

Again, a body in Scotland, the Faculty of Physicians and Surgeons in Glasgow, have, on the strength of an old charter, exercised the right of licensing candidates to practise surgery within the four counties of Lanarkshire, Renfrewshire, Dumbartonshire, and Stirlingshire; and this they did for a long series of years upon a very limited scale of education. The persons so licensed receive no privilege to practise beyond the precincts of the counties now specified; yet it is known that several of them have exercised the art, without let or hindrance, in other parts of the United Kingdom.

A degree of irregularity and variety most hurtful to the public interest is thus sanctioned.

Besides the sources of disparity and variety thus specified, graduates from various Scottish universities, especially Edinburgh, have appeared as candidates for general practice in various parts of the United Kingdom, and have claimed right so to practise by virtue of the possession of the degree. Indeed, by far the largest proportion of those honoured by this degree begin their professional career as practitioners of all-work, sinking in the meantime the academical title, at least not bringing it prominently forward; and after having become rooted in a good locality, and acquiring a certain degree of name and confidence with the public, have at length, by gradual steps, sometimes more suddenly, ascended the heaven of invention provided by the degree. On the merits or demerits, the propriety or impropriety of persons with degrees practising as general practitioners, we refrain from giving any opinion. The fact alone we state; and while we do so, we state it to demonstrate the same fact of inequality in education possessed by candidates licensed by different bodies. The graduate of Edinburgh has long been at the head of all those educated in Scotland, excepting the licentiates of the College of Surgeons, who have at some periods been enjoined a more full education, and who have at all times kept close upon them. The graduates of Glasgow, again, and Aberdeen have in general had a much less extended course of education; and we are not aware that either of them have come up to the pupils of the junior college, unless in the circumstance of Aberdeen requiring a much longer period of hospital attendance than either of the others.

Even the greatest enemies of the school at Edinburgh have virtually admitted the superiority of the course of education there prescribed, in adopting or imitating of late years the most valuable parts of the system pursued in that place. Still the disparity continues great, and productive of much evil. Thus, neither the College of Surgeons of London nor the Apothecaries' Hall require of their condidates attendance on separate substantive courses of Institutes of Medicine or Physiology, as far as we are aware, nor in Clinical Medicine, nor in Clinical Surgery; the former the most important in the elementary parts of education; the latter, the most important in the practical department. Theory of Medicine or Institutions are taught either as part of the lectures on practice of medicine along with anatomy. At present, we believe so predominant is this fancy of uniting subjects which the state of science and the efficiency of education require to be separated, that the authorities at Cambridge refuse to admit to examination, candidates who do not possess the ticket to a course of lectures on Theory and Practice of Medicine.

It must not be concealed, nevertheless, that in this system of varied training, various compensating principles are observed to operate, which modify, if they do not counteract, the irregular results. Thus, while the English Boards require not the formal clinical courses of the Scottish school, they insist upon a lengthened attendance on the medical and surgical practice of hospitals, expecting thus to insure, by a more lengthened attendance, the same results as are obtained by the system of clinical instruction. In the first mode the pupil is expected to do for himself what in the second he is taught to do under the guidance of the hospital physician. It is, perhaps, of little moment, it may be argued, in which mode the information is acquired, if it be acquired, and perhaps each has peculiar advantages.

In the mode of educating graduates, or those destined to the highest rank in the profession, considerable inequality is observed. To that prescribed for those of the Scottish universities, we have already adverted. The education of the graduates of the two English universities and of Trinity College, Dublin, differs still more widely from that of the Scottish institutions, both in the amount and kind of acquirements, and in the mode of obtaining them. The statutes of the two English universities demand a lengthened and full course of preliminary literary education, which must be obtained by residence at one or other of the universities, and by keeping terms as prescribed by academical and college rules. But, as the medical department in both universities is limited, and the professors have little inducement to deliver full courses, the students are allowed to seek, wherever they can best find it, that amount and kind of professional knowledge which cannot be procured within the precincts of the university. It is manifest that no medical school can be efficient without having a large hospital connected with it, and ample means of anatomical instruction; and as these advantages can concur nowhere except in large towns, Oxford and Cambridge, in which the population was not great, remained long without these necessary adjuncts of an efficient medical school. This deficiency it was always easy to supply by allowing the students to spend part of the time assigned for professional study in London, Paris, or any other large city where hospitals abound, and the means of anatomical instruction are easily obtained. Accordingly the majority, it may be said the whole of those who were destined to obtain the fellowship of the College of Physicians in London, received their professional education in London, sometimes with a season or two in Paris or Edinburgh.

It is impossible to deny that this arrangement, combined with the lengthened period of study, makes ample provision for insuring to the graduates of the English universities a most complete, extended, and correct education ; and hence the College of Physicians of London have at all times numbered among their ranks the most learned, intelligent, and accomplished men in the profession, making, of course, all due allowance for the varieties and inequalities produced by individual character, talent and disposition. They have almost always had, or been supposed to possess, the advantage in literary and classical education,—a matter certainly of the utmost importance in forming the character of an accomplished physician.

It has become fashionable, in these days of levelling utilitarianism, to sneer at and depreciate the pursuit of such studies, as of no use to the physician, either in the diagnosis or the treatment of diseases. He must be a poor reasoner, however, and a very narrow-minded observer, who employs an argument of this kind, or argues that, because direct benefit cannot be expected from the possession of a liberal education in the pursuit of the medical profession, therefore the benefits of literary training are naught and worthless. Are there, we ask, no indirect benefits ? Are not the mental faculties cultivated and polished ? Does not the mind receive a better and more elevated tone ? And will individuals not thus be more likely to be raised above those debased and degrading arts which have been the reproach of mere traders in medicine ? Classical learning can impart neither judgment, nor common sense, nor talent, nor moral principle, where they are wanting. But it clears the one and adorns the other, and qualifies its possessor for directing his mental powers to the proper objects of professional study with a degree of energy, method, and effect, which can be procured by no other adjuvant. Various silly and irrational notions are entertained on the utility or the uselessness of classical learning, all of which only show the total unfitness of those who have given opinions on this point to judge of it at all. We have heard the knowledge of the ancient languages commended to the physician, because it would enable him to understand the derivation and signification of the terms used in his science. This is doubtless a reason for some knowledge of them; but it is a reason most absurd, puerile, and mechanical; and, were it the sole reason, the study of these languages might be given up to-morrow. It is the very discipline which the mind receives in the study of these languages, the habits of precision and accuracy in thinking and speaking, the general enlargement of mind and elevation of sentiment, that confer the great and lasting benefits on those so trained. The sneers and sarcasms indulged in by the utilitarian party at the attention paid to classical pursuits at the English universities, especially the most ancient of the two, may be left to the silence they merit, when it is remembered what substantial services have accrued to the cause of Christianity and pure religion from this devoted attention to the treasures of ancient literature. When we think on the writings of such men, as Hooker, Taylor, Bull, Barrow, John Smith, Wollaston, Butler, Warburton, Samuel Clarke, and Jortin, is it possible to give a moment's countenance to such a very shallow paradox as that the study of the ancient writers of Greece and Rome is useless, or that the time spent in acquiring some knowledge of them is wasted? In studying the ancient languages these men became powerful and learned reasoners, sound and rational theologians, and strengthened the cause of religion, natural and revealed, with their most unanswerable arguments. They demonstrated the influence, direct and indirect, of sound learning and knowledge on their own professional pursuits. The connection may not be so direct between classical learning and the pursuits of the physician; but a connection there is, nevertheless, and the influence might be easily demonstrated. The argument we do not wish to carry beyond its legitimate limits. But we maintain that, to the well-informed and accomplished physician, the study of the ancient languages among preliminary and collateral pursuits is of the highest importance, and indeed absolutely necessary. It is therefore our deliberate opinion, that in this point the English universities and the College of Physicians of London are decidedly in the right; and, though inequality may thus be produced, it must be diminished, not by lowering the standard fixed by the English universities, but by raising other bodies to that standard.

A second source of complaint in the practice of medicine and surgery as a profession, arises from the operation of monopolies and exclusive privileges enjoyed by particular corporations and licensing boards. We have already had occasion to refer to one form of monopoly,—that enjoyed by the College of Physicians of

London, chiefly by reason of its connection with the universities. This has been denied and disavowed, as many other evils springing from the same source have been disavowed, by Sir George Tuthill, in his able, but partial vindication of the principles and practice of the Royal College,* (XIII.) Sir George answers, that "no man can claim the right of being admitted a fellow; that no man is excluded from the fellowship because he is a Dissenter; that neither fellows nor licentiates are required on their admission to subscribe to any religious creed, or are asked any questions regarding their religious opinions; and that, though graduates of Oxford and Cambridge are obliged to be members of the Established Church by the laws of the English universities, yet, under the existing bye-laws, several physicians have been elected fellows who conscientiously dissented from the English Church, and an avenue to the fellowship of the college is open through the order of licentiates to persons of every religious sect." So far this is true. But the means of admission here pointed out by Sir George have not only been in practice excessively narrowed, but they have been so clogged with impediments, disabilities, and discreditable distinctions, that it has amounted to a prohibition.

There is, indeed, here, first, the restriction and exclusion of all but graduates of the English universities from the fellowship of the college; then the admission to the rank of licentiate itself, accompanied with the brand of inferiority; and, thirdly, there is the sparing and jealous selection of fellows from the body of licentiates,—an act which it is quite impossible to perform in any but an invidious way. Do these proceedings tend to the good of the lieges? Do they tend to furnish them with the best advice and attendance that can be procured? Do they tend to foster sentiments of peace, friendship, and brotherly love in the profession? To these questions we fear that there can be only one answer; and that answer could not leave the college free from the charge of a corporation, monopolising and exclusive spirit.

We willingly admit, as we have already done, the great advantages accruing to physicians, and, of course, to the profession, from the long and full academical education which the preference to Oxford and Cambridge graduates certainly does secure to the fellows of the college. We admit the beneficial effects of the lengthened and diligent mental training which ought to result from a residence at Oxford or Cambridge; we know, from personal observation, that it does not always result. We even admit the probable beneficial effects of the moral and religious training which ought to result from breathing the orthodox air of the English universities for a series of years; and, beholding these venerable institutions with the reverence with which we have ever done, as the great external means of maintaining the purest and most rational form of the Christian religion,—free from fanaticism on the one hand, and latitudinarian scepticism on the other,—we believe that residence at either for some years in early life is as well calculated as any merely human contrivance can be for impressing indelibly on the heart the great principles of morals and religion. Beyond this, however, we cannot proceed. While we cannot deny that in Scotland classical education has been more neglected than it ought to have been, and in no department more than in the education of the medical profession, we think the exclusion from the fellowship, merely because Virgil and Homer, Terence and Sophocles have not been studied on the classical banks of the Cam or the Isis, is at once harsh, illiberal, and unjust. If candidates give proof of sufficient learning and knowledge, is it of any moment where the information has been acquired?

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* Vindiciæ Medicæ, or a Defence, &c. London, 1834.

Regarding the alleged "acquisition of that intellectual and moral condition which is best adapted to the pursuit of philosophical, and to the religious observance of all social duties," which is said to be insured at these universities, we are sorry to perceive a fellow of the college talking in a style so puerile as to border on the ludicrous. Is an intellectual and moral condition ever acquired in this manner? is it acquired, if such conditions of our being can be said to be acquired, by residence at any university? Multiplied observation and experience must answer, that if already possessed, it is well, if it be not lost, or at least greatly impaired during residence at any university: No, no, Sir George; a young man must have gotten these invaluable treasures before he goes thither, if he is to have them at all, and he may, if firm and decided, resist all temptations to part with them. The true scene for the developement and growth of moral and religious principles, is the paternal roof; and there also, most commonly, the first rays of intellect are most successfully brought out. It is the example and precept of the parents, and especially of the mother, that are most effectual in fostering, guiding, and directing these great principles of our nature.

Of great use and efficacy, we grant, in preserving and confirming these principles in the English schools and universities, is one custom observed there, at least since the Reformation; and we must ever look on it as a great defect, that some practice analogous or the same is not punctually observed in the Scottish establisments. We allude to the practice of reading prayers morning and evening, and the psalms and lessons for the day. It cannot be doubted that of this there is too little in the Scottish schools and universities; and that, to the external observer, the pupils appear to be allowed to pursue their studies, without any particular time being devoted to the performance of religious duties. At the same time, when we know the careless, cold, and hasty manner in which the chapel duty used to be performed in the English universities; when we know also the little effect that these solemn duties exercise on the life and conversation of the students, it is impossible to resist the inference that this excellent practice has not been productive of all the good effects which its original founders expected it would.

But under all circumstances, every impartial judge will admit that it is at once idle and unreasonable to claim for the universities alone, that moral and religious training which Sir George Tuthill does, and which looks as if he imagined that all pure morality and undefiled religion were monopolized by the universities, much as the correct knowledge of the Greek and Roman authors is monopolized. Wherever the Scriptures are read and the precepts of the New Testament reduced to practice, morality and religion will be inculcated, whether within or without the walls of a university.

As to the other argument by Sir George, that " the social relations under which the physician is placed, the delicate and complicated conditions under which he is perpetually invited to offer some relief to human misery, render it equally imperative that the highest sense of honour and probity should be demanded and insured," it is a truism stated with such absurd pomp and formality, that it would almost lead to the opposite inference, that neither the one nor the other of these qualities were to be found among the graduates of the English universities; and we are irresistibly led to the inference, that mere classical learning, even within the walls of a university, can neither impart judgment nor common sense, nor furnish the power of reasoning where it is wanting. But seriously, if these qualities do not already exist in the minds of the pupils, by what university in the civilized globe can they be communicated?

All these evils and grievances, and several others closely connected with them, have been felt so keenly for a long time, that, at length, after the profession had been long dumb with silence, and refrained even from good words, their sorrow was stirred, and the fire, which had long smouldered in their hearts, broke forth. A petition, containing a statement of their grievances, and exhibiting the oppression, injustice, and exclusive spirit of the Royal College, signed by forty-nine physicians, all most respectable, and some of high name and character, was presented in 1834 to both Houses of Parliament. This petition was couched in very temperate language, and in a respectful style, and called forth an elaborate vindication by Sir G. Tuthill, as already stated. It produced no immediate effect. Nothing of the kind ever does. But sagacious observers could discern remote results. It was quietly suggested that several of the physicians stigmatized as licentiates, would be admitted to the fellowship if they chose to apply; and the fellowship was even understood to be offered to some of the petitioners, who of course refused to accept it unless upon more gracious conditions, and unless the offensive regulations regarding admission were repealed or modified. Many new fellows were soon after admitted, partly to show the liberal spirit of the college, partly to conciliate the foe; and, it may be added, also to denote their approbation of talent and merit. These proceedings have been since followed by others, which indicate the dawn of a more kind and liberal spirit in the college; and it is gratifying to observe that the present fellows of the college have evinced a most sincere desire to remove all hurtful impediments to the privileges of the body, and to abolish all unnecessary and invidious distinctions.

The College of Surgeons has in like manner been charged with the exercise of restrictive and exclusive privileges. But from the particular nature of the business of surgeon, consulting, operating, or practising, the exercise of these exclusive privileges, though sufficiently offensive to certain individuals, has never been productive of such general mischief and such widely diffused discontent as that of the sister college.

The chief source of complaint against the College of Surgeons, is that, whereas every one applying for their diploma, and qualified to receive it, becomes a member of the body, he has no right to interfere in its government, internal constitution, or the management of its affairs. These duties or privileges are confined to twenty-one members, denominated the court of assistants, who are appointed for life, and who possess the power of nominating their successors. It appears that, without going back to the remote period of Henry VIII., when the surgeons were first united in a corporation with the barbers, by the 18 George II. they were erected into a distinct corporation; that sixteen individuals were named by government; and that these were authorized to appoint five others, thus forming the court or council of assistants, who were then empowered to fill all vacancies in the offices of the college, and to conduct the whole internal administration of the body. The members of the college were estimated, in 1836, at between 8000 and 9000 persons. Probably at present they are not fewer than 10,000, all of whom must be regarded as integrant parts of this body, whose affairs are managed in London by twenty-one persons.

It might appear at first sight that, to the great body of the 10,000 diffused through the counties of England and Wales, all occupied more or less fully in professional pursuits, several attached to provincial hospitals and infirmaries, others to various charitable institutions, it was a matter of perfect indifference by whom or how the affairs of the college were managed. It is clear that if these provincial surgeons attended to their own affairs, they could not, even in these days of railway conveyance, attend to those of the college. That these persons did not therefore care about the matter, we believe. It must have been chiefly the members in and about London who felt the grievance ; and if the former ever were brought to take an interest in the matter, it must have been in sympathy for their London brethren.

To the latter the great cause of complaint was the first bye-law of the college ; which enjoins,—" That no member of the college, whose professional practice is not confined to surgery, shall be elected a member of the council." To understand the operation and bearing of the law, it is necessary to explain, that the terms surgeon and surgery bear different meanings at different times and with different people. Literally and originally, surgery (*Chirurgia*) means manual operation ; and to this anciently and often in modern times it was strictly confined ; the surgeon being anciently often a slave who was desired by the physician to perform certain operations ; and in the middle ages, and long after, being a person so humble and degraded, that he waited the orders of the sacerdotal physicians to perform such operations as they thought necessary for the recovery of their patients. In modern times, in the hands of Cheselden, Sharp, Bromfield, Pott, John Hunter, Abernethy, John Pearson, Sir Astley Cooper, Sir Benjamin Brodie, Mr Travers, and Mr Lawrence, surgery means the knowledge of the nature of those local and external diseases which admit of treatment either by general and local means, or when these fail, by manual operation where admissible, and the knowledge of the appropriate means of cure. This is pretty comprehensive ; but we believe it is free from error, and would not be disavowed by the most rational surgeons of the present day. Within the last twenty or twenty-five years, however, a new light has dawned on this subject. Surgery has been distinguished into surgery and pure surgery ; and surgeons have accordingly been distinguished into the order of surgeons and pure surgeons. What is meant by pure surgeons, we are not sure that we understand very clearly. But we believe the term is intended to be applied to those who confine themselves in practice to the treatment of surgical diseases solely, and the performance of operations, surgeons being that less discriminating body, who, like their forefathers, treated all and sundry maladies and accidents with equal readiness,—medical diseases, surgical diseases, the puerperal state, and the diseases incident to females and children. Here, however, it must be remarked that, while midwifery practice was openly rejected and medical practice coldly coquetted with, by the purists, the latter was nevertheless patronized in a quiet way, and formed, it is well known, the great source of the professional emoluments of these pure and self-denied persons. Wounds, bruises, fractures, mutilations, amputations, excisions, lithotomy, and similar sanguinary operations, form but a small proportion of the calamities and sufferings of the human race; and we believe the best judges admit that no surgeon scarcely could live on his mere operating practice. This, of course, is the ordinary state of human affairs. When, of course, mankind take to cutting, stabbing, and shooting each other, on the great scale, which happens of course every now and then, a different state of matters is presented. Every possible variety of injury then occurs, and requires something to be done for it. But even then not much is required to be done long. Mankind have been so successful in bringing to perfection the art of destroying their own species, that after a battle has been well and heartily fought, little is left for the surgeon to do.

If the purists intended therefore by this regulation to exclude all but themselves, we think they must have been labouring under a great delusion. We, indeed, doubt whether there be such an individual in the whole college as a pure surgeon according to their definition; for certainly it must have been impossible to refer to this category even the hospital surgeons, all of whom, it is well known, had much more practice as medical practitioners than as operating or even consulting and attending surgeons. They were indeed physicians and medical practitioners without the title of graduate or the seat in the College of Physicians. The truth is, that the high name which a skilful and dexterous surgeon acquires, makes society believe that he is as skilful in the recognition and treatment of internal as of external or local diseases; and he is consequently consulted much oftener in the former than in the latter, because these constitute the great mass of complaints for which assistance is required; and he is consulted more frequently than the physician, because somehow or other, the public have gotten an idea, that the physician should never be summoned except in extraordinary cases, and when danger is so urgent, that too often all hope of doing good is over. So at least has it been in fact, and so probably will it ever be.

The complaint already referred to, though apparently not productive of serious inconvenience, for it certainly did not interfere with the exercise of other privileges, has recently induced the council to admit to the general privileges of the college a considerable number of surgeons, selected by them in London and the provincial towns. Yet the mode in which this has been done has given rise to feelings of envy, jealousy, and discontent, and at present it appears that the latter course of the college is much more offensive than the oligarchy of former days.

A more intelligible ground of complaint we have heard laid to the charge of those who were hospital surgeons, though we do not vouch for the truth or correctness of the statement; neither can we tell in what mode, or how far it is connected with the mysterious movements of college politics. It had been long asserted that the surgeons of the leading hospitals in London, who were almost invariably, actually, or had been in the court of assistants, contrived to make a sort of monopoly of the hospitals in favour of their own pupils or apprentices. The most likely mode, indeed, it was said, the only one, for any surgeon to become attached to any given hospital, was to begin by being apprentice to one of the surgeons of that hospital; and that, in the subsequent disposal of these appointments, the claims of all other candidates are so disregarded, that to apply is needless.

To the inequality of education permitted by the College of Surgeons we have already adverted.

The third body who has recently claimed, and been allowed exclusive privileges and restrictions detrimental to medical practice is the Society of Apothecaries. But we have already, on several occasions, spoken of the effects of the corporation privileges of this body so much, that we shall not spend more time on the subject.

There are still, nevertheless, other bodies who have, on various pretexts and in different modes, exercised monopolies more or less complete, and more or less absolute, and, by means of which, they have contrived either to regulate and exclusively conduct the education of at least all graduates, at one time, and in certain places, of all medical students, or they have thrown such impediments in the way of medical education being obtained wherever it best can, from other teachers, however well qualified, that several of them have contributed very largely to the grievances of the profession. These are the universities.

Of the Universities of Oxford and Cambridge we have already spoken; and of the curious effects of their exclusion from honours of all who did not or could not subscribe the thirty-nine articles. As institutions ecclesiastical in origin and connection, and governed entirely by clergymen of the Church of England, they may have the full and absolute right of making and enforcing such a statute. But the prudence of such a measure, as regards either themselves as didactic institutions, or the public, who are to gain or lose by such an ordinance, may be justly questioned; and the justice of the ordinance is still more doubtful when its influence on admission to the College of Physicians is considered. We believe proposals have been brought forward at Cambridge, which has always been distinguished for a certain degree of liberality, to effect some relaxation in the severity of this enactment. But we have not yet received correct information as to whether they have been carried into effect.

The graduates of the two English universities have alone the right of practising in England, anywhere beyond the radius of seven miles, without license from the College of Physicians. To all graduates of any other universities it is indispensable to obtain the license.

The power of regulating the education of graduates, claimed by or allowed to the universities, has been a fertile source of evil. As it remained with these bodies to say how much or how little medical education a man might possess, how long or how short he might study, and where the education must be taken, and different universities were satisfied with different measures of knowledge; not only was irregularity thus produced, but, when the candidates were obliged to pursue these studies at a particular medical school, much inconvenience and loss of time, as well as loss of money, was not unfrequently caused.

It has been already shown in what mode the English universities proceeded in this matter. Though they enjoined university residence for obtaining literary and philosophical distinctions, and the regular and punctual keeping of academical terms, their candidates for the degree of bachelor or doctor in medicine they allowed to procure their professional information wherever it could be obtained of the best quality, and in the most complete manner. Residence at the university for this purpose was not enforced; and the hospitals and courses of lectures delivered in the metropolis and elsewhere, if by qualified teachers, were and are allowed by these bodies to stand for good and efficient instruction.

The same course has been followed by some other universities, the United Colleges of St Andrews, for example, and, if we mistake not, for part of the studies, King's and Marischal Colleges, Aberdeen. Others, for example the College of Edinburgh, require the whole of the courses prescribed to be attended within the walls of a university, and delivered by a professor; and any instruction not communicated by one of these privileged persons is regarded as useless and esteemed as nothing. The only exception made is, that, of the four years during which medical studies must be prosecuted, one may be taken, not at a university, provided the candidate attends at the same time the practice of an hospital containing more than eighty beds, and studies practical anatomy. This does not, however, exempt him from attending every one of the classes prescribed. The effect of this is, that, in the short space of three years or twenty-seven months, the student may have to attend lectures on at least thirteen, it may be, fourteen different subjects. It may be safely asserted that no person of ordinary diligence and the average amount of intellect can do this at all, certainly not with profit. The thing is a psychological impossibility. To study so many subjects with the attention which they demand, and with the hope of understanding them, the period of study ought to be lengthened.

Less exorbitant in her demands, the University of Glasgow requires attendance on eight separate courses only, and dispenses with Natural History, Medical Jurisprudence, Clinical Medicine, Clinical Surgery, Practical Anatomy, and General Pathology. When the Medical Faculty of Glasgow first promulgated this curriculum, they added the condition that all the courses must be delivered by a university teacher. But the severity of this regulation they relaxed in favour of the lecturers in London and Dublin, whom they thus announced to be much superior and more accomplished teachers than the lecturers either in Edinburgh or Glasgow. The members of the faculty being gentlemen of judgment and experience in matters of this kind, are probably in the right. It is a practical commentary upon the text *omne ignotum pro magnifico.* But it further shows what curious tricks in medical legislation are performed when the teachers alone are examiners and are allowed to legislate.

This invidious and partial method, however, of relaxing the strictness of academical regulations in favour of two bodies of lecturers situate at a distance, while they are retained in all their original force as to those more near the university, bad as it is, is much less hurtful than the system of absolutely and positively enjoining the whole of the courses required, to be taken from university lecturers. This monopoly has at all times formed one of the most offensive and oppressive of the university privileges, and that which has been productive of the greatest amount of evil. Its effects have been long and repeatedly the subject of complaint; but they were first brought prominently forward by Adam Smith, who, in considering the claims of certain educational institutions to public support, found reason to pronounce a most severe censure on university privileges and monopolies.

"Whatever," says Dr Smith, "forces a certain number of students to any college or university, independent of the merit or reputation of the teachers, tends more or less to diminish the necessity of that merit or reputation."

"The privileges of graduates in arts, in law, physic, and divinity, when they can be obtained only by residing a certain number of years in certain universities, necessarily force a certain number of students to such universities, independent of the merit or reputation of the teachers. The privileges of graduates are a sort of statutes of apprenticeship, which have contributed to the improvement of education, just as the other statutes of apprenticeship have to that of arts and manufactures."

"If, in each college, the tutor or teacher, who was to instruct each student in all arts and sciences, should not be voluntarily chosen by the student, but appointed by the head of the college; and if, in case of neglect, inability, or bad usage, the student should not be allowed to change him for another without leave first asked and obtained; such a regulation would not only tend very much to extinguish all emulation among the different tutors of the same college, but to diminish very much, in all of them, the necessity of diligence and of attention to the respective pupils. Such teachers, though very well paid by their students, might be as much disposed to neglect them as those who are not paid by them at all, or who have no other recompense but their salary.

"If the teacher happens to be a man of sense, it must be an unpleasant thing to him to be conscious, while he is lecturing his students, that he is either speaking or reading nonsense, or what is very little better than nonsense. It must, too, be unpleasant to him to observe that the greater part of his students desert his lectures; or, perhaps, attend upon them with plain enough marks of neglect, contempt, and derision. If he is obliged, therefore, to give a certain number of lectures, these motives alone, without any other interest, might dispose him to take some pains to give tolerably good ones. Several different expedients, however, may be fallen upon, which will effectually blunt the edge of all those incitements to diligence. The teacher, instead of explaining to his pupils himself the science in which he proposes to instruct them, may read some book upon it; and if this book is written in a foreign and dead language, by interpreting it to them into their own, or what would give him still less trouble, by making them interpret it to him, and by now and then making an occasional remark upon it, he may flatter himself that he is giving a lecture. The slightest degree of knowledge and application will enable him to do this without exposing himself to contempt or derision, of saying anything that is really foolish, absurd, or ridiculous. The discipline of the college, at the same time, may enable him to force all his pupils to the most regular attendance upon this sham lecture, and to maintain the most decent and respectful behaviour during the whole time of the performance.

"The discipline of colleges and universities is in general contrived, not for the benefit of the students, but for the interest, or, more properly speaking, for the ease of the masters. Its object is, in all cases, to maintain the authority of the master, and, whether he neglects or performs his duty, to oblige the students in all cases to behave to him as if he performed it with the greatest diligence and ability. It seems to presume perfect wisdom and virtue in the one order, and the greatest weakness and folly in the other. Where the masters, however, really perform their duty there are no examples, I believe, that the greater part of the students ever neglect theirs. No discipline is ever requisite to force attendance upon lectures which are really worth the attending, as is well known wherever any such lectures are given. Force and restraint may, no doubt, be in some degree requisite in order to oblige children, or very young boys, to attend to those parts of education, which it is thought necessary for them to acquire during that early period of life; but after twelve or thirteen years of age, provided the master does his duty, force or restraint can scarce ever be necessary to carry on any part of education. Such is the generosity of the greater part of young men, that so far from being disposed to neglect or despise the instructions of their master, provided he shows some serious intention of being of use to them, they are generally inclined to pardon a great deal of incorrectness in the performance of his duty, and sometimes even to conceal from the public a good deal of gross negligence."

"In England the public schools are much less corrupted than the universities. In the schools the youth are taught, or at least may be taught, Greek and Latin; that is, every thing which the masters pretend to teach, or which, it is expected, they should teach. In the universities, the youth neither are taught, nor always can find any proper means of being taught the sciences, which it is the business of those incorporated bodies to teach. The reward of the schoolmaster, in most cases, depends principally, in some cases almost entirely, upon the fees or honoraries of his scholars. Schools have no exclusive privileges. In order to obtain the honours of graduation, it is not necessary that a person should bring a certificate of his having studied a certain number of years at a public school. If, upon examination, he appears to understand what is taught there, no questions are asked about the place where he learnt it."*

The general principles now announced are perfectly applicable to the whole of that system of regulations by which medical students, who aspire to the honours of graduation, are obliged to study the departments of medical science at universities, and under the auspices of university teachers. Indeed Dr Smith, in his able letter to Dr Cullen on Medical Reform, expressed the decided opinion, that any regulation of that kind which should exclude such private teachers as the Hunters, Hewson, Fordyce, &c. would be oppressive and unjust. Yet Dr Cullen had only proposed that no person should be admitted to examination for a degree, unless he brought a certificate of his having studied at least two years in some university. "The scholars," says Dr Smith, "of such teachers surely merit whatever honour or advantage a degree can confer, much more than the greater part of those who have spent many years in some universities, where the different branches of medical knowledge are either not taught at all, or are taught so superficially, that they had as well not be taught at all. When a man has learned his lesson very well, it surely can be of little importance where or from whom he has learnt it."

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* Wealth of Nations, Book V. Chapter i. Part iii. Article ii.

What would have been the judgment of Dr Smith if he had lived to see proposed and enacted the rule of enforcing four years of attendance on the instructions of university teachers, and excluding the instructions of all not university teachers, however well-informed and however industrious?

"The monopoly of medical education," adds Dr Smith, "which this regulation would establish in favour of universities, would, I apprehend, be hurtful to the lasting prosperity of such bodies corporate. Monopolists very seldom make good work, and a lecture which a certain number of students must attend, whether they profit by it or no, is certainly not very likely to be a good one."*

It is impossible to deny that these views are to a very great extent well founded, and their correctness has been confirmed by some experience. It would not be difficult for the experienced in these matters to adduce from their own knowledge, instances of institutions where certain departments of medical science are taught in a manner so void of method and order, with so little knowledge of the most recent and correct doctrines, and with so little regard to the essential and important parts of the subject, that it has ceased to be understood, that students can, at such places, acquire even a moderate knowledge of the subject. In one of these privileged establishments, for example, the teacher to whom is assigned the office of teaching anatomy, appears to imagine that it is quite unnecessary to go over the elementary parts of that important science in a systematic or methodical manner. Instead of correct demonstrations of the component parts of the human body, or lectures on the same subject, the course is made to consist of a little anatomy, a little morbid anatomy, some animal chemistry, some equivocal physiology, some pathology, some obsolete surgery, and even some practical medicine. But in the meantime the great objects of the lectureship are completely and absolutely sacrificed to so great an extent, that it is not expected that students can there acquire any useful knowledge of anatomy.

So severely have evils of this kind been felt that all sorts of expedients have been thought of for obviating or removing them. A controlling power for removing inefficient teachers even has been talked of by the more violent and uncompromising of those, who are impressed with the detrimental effects of such abuses. But this, to say the least of it, would be a harsh and ungracious measure, and one which might easily be converted into an engine of oppression and persecution. It would not be always easy to define inefficiency, or to specify the degree of remissness which might justify supersession. In some instances the blame of such evils has been imputed to the mode of appointment. The patrons, it has been asserted, are incompetent judges; understand not the true interests of the establishment; and are totally indifferent to the responsibility of making the proper selection. But it may be answered, no mode of appointment can secure against the occurrence of such evils. A man may be active, industrious, and attentive when first appointed, and may afterwards turn out to be useless, indolent, and irregular in the performance of his duty.

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* Life of Dr Cullen, by Dr Thomson, Vol. i. p. 474.

The simplest and most effectual, and the most obvious and practicable means of counteracting the evils now adverted to, is manifestly to adopt something of a plan suggested several years ago by the committee of the Colleges of Physicians and Surgeons here, viz. to remove the necessity of obliging the students to attend *all* the branches of medical education prescribed at universities, and to allow them to pursue the study of certain branches wherever, or under whomsoever, they can do so most profitably.

From the view of the grievances, evils, and abuses now adverted to, it is clear that the profession of medicine and surgery could, as to irregularity in education, and the confused and conflicting, but always hurtful operation of the privileges of particular boards, scarcely be in a worse state, and that some means of amendment, rectification, or reform were most urgently demanded. These evils, indeed, were so acutely felt, that, after meetings, petitions, remonstrances, and letters from various bodies of medical practitioners urging the necessity of reform, and, among others, the petition of the forty-nine licentiates, in 1834, on the motion of Mr Warburton, a select committee was appointed in the House of Commons "to inquire into and consider of the laws, regulations, and usages regarding medical education and practice of the various branches of the medical profession in the United Kingdom, with power to report the minutes of evidence taken before them." The labours of this committee were unquestionably most beneficial, not only in collecting the evidence and testimony of many different corporate bodies, colleges, universities, and medical schools, but of many well-informed individuals who had made this subject their particular study. After sitting for several months a great body of evidence was collected. That regarding the English colleges and schools alone has been printed. That regarding the Scottish establishments, it is feared, has been lost or destroyed. At all events, it has never been printed.

It was always understood that the evidence thus collected was to be made the foundation of any attempts at legislation, which might be considered requisite, with the view of reforming and improving the state of medical education and practice. Accordingly, two different bills were brought forward, one by Mr Warburton, and another afterwards by Mr Hawes, neither of which, however, were found capable of being persevered in, so as to be brought into committee. Both, in short, were tacitly abandoned. Soon after the accession of Sir James Graham to the office of Secretary for the Home Department, it was understood that, in consequence of a very general wish entertained by all the leading bodies in the profession, an attempt would be made to bring forward such a legislative measure as the circumstances of the case would justify. At first it was believed that a draft of this measure would be presented in the course of the session of 1843. But that session was so crowded with various matters of public business that it was allowed to pass over without anything being matured. At length, however, after many delays, and at a late period of the session 1844, Sir James Graham brought forward, on the 7th of August, the bill, the title of which is prefixed to this article, and a copy of which is given in our Medical Intelligence. Of the different provisions of this bill we shall possibly take occasion to speak more at length on another occasion. All that we propose to do at present is to say a few words on its general principles, and on its aptitude for removing the various grievances which have been so long and so severely felt by the profession.

And in the first place, it might be regarded as a good omen of the general merits of the proposed bill, that, though brought forward by Sir James Graham, and understood to be, to a certain extent, therefore, a government measure, it was warmly and cordially supported by Mr Macauley, Mr Warburton, and Mr Hawes, the first, certainly the ablest, and most enlightened and judicious of the opposition leaders, and the other two, the authors of the bills which had formerly been found inapplicable, or otherwise incapable of being carried through Parliament, and likewise opposition members. The only appearance of opposition was made by the member for Finsbury. In the second place, it was known that Sir James Graham had been long at considerable pains to ascertain the amount, and the kind, of the wants of the profession, which any legislative measure might be expected to supply; and we believe that the main cause of deferring the measure from 1842, when some expectations of its appearance were entertained, to 1844, when it did appear, was to avoid the chance of hasty, crude or precipitate propositions, and to present a measure which might be the result of deliberate and careful consideration of the subject, and serious and sincere desires to meet the actual exigencies of the case.

Notwithstanding these circumstances, which were either well known or could be easily learnt, this bill became very speedily the subject of a considerable amount of criticism, censure, and opposition. The criticism has not been judicious, and has been almost invariably rash and hasty. The censure was often either unfounded, or originated in erroneous views and imperfect information. And the opposition which has been rising, both in the profession and out of it, has been also premature and most injudicious to say the least, and in some instances it has appeared to us to be perfectly factious and frivolous. It is quite clear, that considering the multiplied evils, grievances, and abuses prevalent in the profession, any sincere attempt to amend or remove these ought to be received at least with patience, thankfulness, and kindness; and in criticising the separate provisions of the measure, it should never be forgotten that the most difficult of all tasks is to legislate where the jarring interests of so many different bodies are to be reconciled, where ancient and acknowledged rights must be respected, and where a great system of machinery requires not to be violently rent asunder, but to be remodelled, amended, and rectified.

Let us contemplate the matter dispassionately and deliberately, and see the conclusions to which we are led.

We have seen that the two great master-evils of the present system, is the inequality and irregularity of education and qualification, and the irregularity of privilege, or rather the existence of privilege in one part of the empire, and the denegation or absence of it in another. No rational person will deny, we presume, that any legislative measure, by means of which these irregularities and inequalities can be effectually removed and uniformity introduced, must produce great and most decisive benefit. Now, the two leading objects of the bill introduced by Sir James Graham are these; *first*, to insure for all the different members of the profession, however much they may be divided into orders, and whatever divisions they chance to cultivate in practice, a certain uniform amount of education, without which no person shall be entitled, legally or publicly, to practise any part of the profession; and along with this, to insure to all those destined to occupy the same professional rank and position in the three kingdoms, the same uniform amount of education; and, *secondly*, after this standard has been fixed, and after the individuals belonging to each rank shall have been publicly certified, to confer on each the same privileges in every part of the British empire. One of the great and powerful recommendations of this bill, therefore, is, that it provides the public with a class of well-informed general practitioners.

To accomplish these two leading objects, every one of the provisions of this bill is directed; and when it is remembered that there are in the United Kingdom not fewer than sixteen examining and licensing, or ruling boards, all at present enjoying certain rights and privileges, sometimes incompatible with those of other similar bodies, it will be admitted that the task is not free from difficulty.

Before entering on the undertaking of new modelling the different boards, certain preliminary steps required to be taken in divesting them of various adventitious incumbrances.

As among the examining, licensing, or superintending bodies, there are some to whom, in their original charters of formation, it was conceived necessary to give certain powers for protecting the regular and licensed practitioners, and discouraging the irregular and unlicensed practitioners, it became necessary to revise and reconsider the whole of these powers. The result has been, that it has been deemed expedient to abrogate all the laws conferring these authorities. This first step has been visited with the most loud and severe censures; and a great clamour has been raised, stating that the author of the bill thereby means to encourage irregular and unlicensed practitioners, in plain English to encourage quacks and charlatans, and to favour the growth of quackery. But surely those who have complained most loudly of this have never given the subject due consideration; and they shut their eyes and close their ears against every thing that experience has brought to light regarding the proceedings of irregular practitioners during the last half century.. Never certainly was clamour more foolish, groundless, and uncalled for. Has it ever been found practicable, by means of penal enactments, to put a stop to quackery, or the existence of quacks? Has any real benefit resulted to the regular members of the profession from the multiplied attempts that have been made to put down irregular practitioners, and the various interlopers by whom the localities of large and wealthy cities are infested. If it were practicable in this manner to put down quackery, by all means we say let it be done. But that it is not practicable, we maintain every impartial observer must at once admit. Prosecute an irregular or unqualified practitioner; he very probably escapes, because it is difficult to prove that he acted as a physician or surgeon; if he is convicted he is instantly converted into a martyr; the sympathies of the public are enlisted in his favour; and he is more consulted and patronized than ever. At all events, it would be only the poor and miserable wretches who would be caught in the meshes of the law. All the large, strong, and powerful animals, in other words, all the most mischievous quacks would assuredly escape, or break through the toils of the law.

Then how is it possible in a free country to make penal enactments, prohibiting my Lord Mortimer, Sir George Harrington, and the Right Reverend the Bishop from consulting Dr or Mr because they are neither of them regularly qualified or licensed practitioners. This would only make these distinguished persons consult such practitioners so much the more. Was it possible to prevent the great, the titled, and the wealthy of the land from consulting Mr St John Long, while that eminent deceiver lived? Or was it possible to prevent Mr St John Long from giving advice to all those who, from being assured that their friends derived advantage from his advice, eagerly sought his skill and attention. That he was eventually prosecuted, and convicted of *mala praxis*, was the result of his own heedlessness and rashness rather than of the wisdom of his enemies. If he had not produced a large irritating slough on the back of a delicate strumous young woman, he might have gone on, pretending to cure consumption, and receiving fees for so doing, at least till he was overtaken, as was actually the case, by that messenger who brooks no delay and who grants no release.

Again, if actions are to be raised and prosecutions instituted, at whose instance are the proceedings to be commenced? Not surely at that of the regular practitioners, who would in this manner be brought into discreditable collision with persons, with whom it must be disgraceful even to contend. It would be creditable to the profession neither as a body or bodies corporate, nor as individuals, to enter into court with such persons. The only way in which, if such a course be thought expedient, must be by relatives of the party believed to be injured applying for redress at common law. Yet with what consistency or propriety can this be done, when the individual or individuals have been allowed to exercise perfect freedom of will in selecting the medical adviser, and must have done so with a full knowledge of his character, pretensions, and qualifications?

We believe, however, that it is not only inconsistent with the spirit and institutions of a free country, to attempt to repress quackery by legislative measures, but that it is impracticable to do so. The public have an inherent right to consult whomsoever they please. When Government have said who are and who are not regular practitioners, the rest must be left to the will of individuals.

We would, in short, recommend all those who have been thinking on this subject to consider this part of the new measure carefully and attentively, and not to be led away by the inflammatory appeals that have been made to them, or by those rash and inconsiderate representations, that any one, however ill-qualified, is thus allowed to practise medicine and surgery. Such is not the object, nor will it be the effect of the bill. Though, according to this provision, the act puts a stop to all prosecutions for ignorance, *mala praxis*, or other pieces of charlatanism, and allows all those who pursue these arts to do so, if they find persons ignorant and weak enough to be their dupes, it directs that all who wish to occupy the rank of regular practitioners shall be well and sufficiently educated, and give satisfactory proof that they are competent to discharge with ability the duties which they undertake.

A manifest benefit resulting from this act is the abolition of all monopolies. While it has been the desire of the legislator to preserve in a state of comparative integrity all the vested rights of the different incorporations, and all those privileges which could be usefully and beneficially exerted for the public good, all exclusive privileges will be abolished; and as all members of the profession will be henceforth educated on the same standard of uniformity, as the lowly and imperfectly educated will be elevated to the proper standard, while those who have means and inclination may rise as much above that standard as they have formerly done, so will the right of practising the profession in its different branches be free to all so qualified and certified, without let or hindrance, from any of those corporate bodies, which have heretofore exerted the right of exclusive occupation of the districts, over which their authority extended. To the particular means by which this will be carried into effect we shall direct attention at a subsequent season.

There is, however, one form of quackery and irregular dispensing, if not irregular practice, to which we think the legislature might properly apply some means of repression, if not positive abolition. We allude to the sale, that is the authorized sale of quack and patent medicines. That these medicines are the instruments of a prodigious amount of mischief we have always been satisfied; and the fact is established by the reports of the Registrar-general, which annually show that not one year elapses without several lives being known to be thus sacrificed, independent of the numbers that are silently terminated by these means and never afterwards heard of. These medicines are, indeed, much more steadily and extensively hurtful than the existence of irregular practitioners. While the latter merely keep the patients much longer ill, than they would be if treated in the regular way, or even if left without treatment, blunder through a series of useless or hurtful applications, and, at the very worst, only kill some unfortunate and credulous dupe once in the year or two, the former are proceeding in a calm, steady, and uninterrupted system of drugging, poisoning, and killing, and most assuredly do more mischief in one week than the irregular doctors do in one year. It is well known that many infants are annually lulled into the last sleep by Dalby's carminative. How many lives are annually offered on the great altar of Morrison's pills? Trials for culpable homicide produced by these medicines take place from time to time; but nothing is heard of the persons who are silently put to death by means of them.

If, therefore, the public authorities really wish to show their sincerity in stopping irregular practice where it can be stopped, this is the point at which to begin. It will be necessary at once to refuse all patents to such medicines the composition of which is kept or attempted to be kept secret. If this be not sufficient, then their sale must be prohibited by express statute.

Our limits and time will not allow us to enter on the consideration of the separate provisions of this bill. But this we expect to be able soon to do; and we hope that we shall then be able to show that these provisions are as well calculated as any legislative measure can be, for removing the grievances under which the different orders of the profession have long laboured. Meanwhile we sincerely think that the present proposition ought to be received with cordiality and thankfulness by the profession, and that its provisions ought to be studied in a careful and dispassionate spirit, instead of being hastily condemned almost without the possibility of their being understood. The general spirit and tendency of the bill is, we think, excellent; and if, in some minor details perhaps, it may be susceptible of improvement, this does not form, in our opinion, an argument sufficient to throw aside the whole measure. Let it be fairly and candidly canvassed; and let all real objections be temperately stated; and we have little doubt that every change will be made that can be expected to be beneficial. We know that the author of the measure has availed himself of every means of information to render the measure as complete and as free from objection as possible, and such that it would be generally acceptable to the members of the profession. To expect that it would be faultless or please every one, is to be unreasonable, and to demand more than it is possible for human capacity or ingenuity to devise. This, however, may be most safely asserted, that, if this bill do not answer the objects in view, it is by no means likely that, for a long time indeed, any measure shall be devised which shall be so likely to do so, and which shall be so free from objection.

We have already shown that it is scarcely possible for the profession to be in a worse state in its social relations than it is at present; that it is disunited in sentiments and mode of operation; that in qualifications there is no uniform standard; that exclusive privileges of different bodies and the monopolies of a few operate most perniciously against each other, and introduce the jarring elements of discord and confusion, animosity and discontent. Surely any measure which proposes to amend these evils and establish something like order, symmetry, uniformity, and regularity, ought to be received with gratitude, and viewed as the harbinger of a more peaceful, and consequently a more agreeable and profitable order of matters. Let this be borne in mind, that whatever tends to improve and elevate the profession, whatever tends to render them more generally respected, must tend in the long run to the advantage and comfort of individuals. Every one will then have the means, under the conditions already prescribed, of displaying his talents and exerting his faculties for the benefit of the public, and to his own credit, in whatever mode he sees most expedient; and by these means it may be expected that all complaints of restriction on the one hand, and undue protection on the other, will gradually but completely cease.

tein are carried by the circulation to the extreme parts, and there, in the act of secretion or nutrition, deposit their oxygen, which unites with matters already existing in the body, and forms the carbonic acid given off during expiration. Further experiments must determine whether, after this, the protein returns by the veins as fibrin or albumen. If the oxides of protein, once formed, do not, in the circulation, resume the forms of fibrin or albumen, then they will serve for the formation of the cellular tissue, of the tendons, of cartilage, of the corneous system, &c., whilst the unchanged fibrin and albumen will contribute to the formation of muscle. In this case the functions of the lungs and liver will be to eliminate from the body those substances taken as aliment which do not furnish protein or its oxides.

On the Temperature of Infants in Physiological and Pathological States. By M. ROGER. (*Comptes Rendus des Seances de l'Academie des Sciences*, 26th December 1843.)—From numerous observations M. Roger ascertained that, during infancy, the natural temperature of the child ranges from 96° to 102° Fahrenheit. During disease, however, the temperature exhibits an oscillation of 34 degrees, varying from 74°.3 to 106°.5 Fahrenheit, according to the nature of the malady. M. Roger hence divides diseases into three groups: 1. Those in which the temperature is augmented; 2. Those in which the temperature is natural; and 3. Those in which it is diminished.

In the first group are included all fevers and inflammatory affections, in all of which the elevation of the temperature is one of the most constant signs. It is a remarkable fact, however, that it is not in the inflammatory forms that the temperature is highest, but in the typhoid fevers. Indeed the main characteristic of even the mildest form of typhoid fever is the production of an immense quantity of caloric. In the second group are arranged dropsies, tubercular diseases, rickets, hooping-cough, &c.; and in the third, œdema, &c.

In commenting on the practical advantage of attending to the temperature of infants during disease, M. Roger remarks, that if the temperature range from 104° to 105°.4 Fahrenheit, with notable acceleration of the pulse and respiration, we may with perfect safety conclude that there exists pneumonic inflammation. Several other examples are referred to in proof of the importance of directing attention to the temperature of infants as an additional means of diagnosis in disease.

On the Causes which determine the Sex in Generation. By M. MOREAU. (*L'Experience*, 4th July 1844.)—Many facts have tended to prove that, in the act of generation, it is the individual who is the strongest who regulates the sex of the progeny. M. Moreau, from long observation, has not only arrived at this conclusion, but thinks that, to a certain extent, a boy or a girl may be got at will by weakening or strengthening the father or mother previous to the act of generation. M. Moreau states that, acting on this rule, he has seen in numerous cases his advice followed by the desired effect.

Influence of Heat and of Stoves on Animal Life. By M. MAGENDIE. (*L'Experience*, 27th June 1844.) The experiments of M. Magendie have demonstrated that the temperature of no animal is capable of being increased beyond 9° Fahr., whatever be the temperature of the air or fluid to which it may be exposed. Thus two rabbits, whose natural temperature is 102° Fahr., were severally placed in stoves, the one heated to the temperature of 140°, the other to that of 212°. After a short while the temperature of both rabbits rose to 111°, that which was in the hottest stove being the first to attain that temperature. In none of his experi-

On the products of the oxidation of Protein in the Animal Organism. By M. Mulder. (*Annalen der Chemie und Pharmacie*, Vol. xlvii. p. 300.) —As the purely chemical details of this long and interesting paper would prove of little value to any but a chemist, we confine ourselves to the important conclusions which M. Mulder draws from them.

When fibrin is boiled in water in contact with atmospheric air, oxygen is absorbed in quantities proportioned to the duration of the ebullition, so that at length the whole of the fibrin is converted into an oxide of protein. It follows from this that when meat is boiled or roasted, the nourishment is not introduced into the organism in the state of protein, but in that of two of its oxides, one the binoxide, being hard and insoluble, the other in the state of the soluble tritoxide. "These two substances," says Mulder, "are both absorbed from the digestive tube unchanged in composition." The tritoxide, or the portion rendered soluble by combination with oxygen, exists in the extract, or soup, of meat. As albumen does not undergo the same changes by ebullition, a tritoxide only being formed, an essential difference exists between giving roast or boiled meat and coagulated albumen as nourishment.

In inflammation, the blood contains a large quantity of the binoxides and tritoxides of protein. The substance which M. Bouchardat detected in the fibrin of blood, and named epidermose, characterized by insolubility in diluted hydrochloric acid, is, M. Mulder states, composed of the binoxide of protein. In fact, M. Scherer has remarked, that fibrin in contact with oxygen absorbs a quantity of that gas, not replaced by an equivalent proportion of carbonic acid gas. He has also remarked that the fibrin of arterial blood, the inflammatory crust, that obtained by beating blood with a stick, that dried in the air, or macerated in alcohol, do not dissolve in the weak solution of muriatic acid; that consequently they consist of the binoxide of protein. In all these, therefore, the protein has become richer in oxygen than when it existed in the state of fibrin. It results from this, that protein may become hyperoxygenated while circulating with the blood. M. Mulder thinks it likely this oxygenation during life is confined to the fibrin alone, and does not extend to the albumen, seeing that when the albumen is boiled it only forms a tritoxide, but no binoxide. These oxides of protein are found in the healthy blood at all times, but are in much larger proportion during inflammation. The hæmatosin of the blood is not, therefore, the only oxygen carrier, seeing that the fibrin at the lungs is converted into the binoxide of protein, (or epidermose of Bouchardat), and into the tritoxide, (or gelatin of Bouchardat). As these principles exist in inflammatory blood in much larger proportions than in healthy blood, it follows that the blood, during inflammation, contains more oxygen, and that a greater amount of oxygen is consumed. It would appear, from M. Mulder's and M. Bouchardat's researches, that it is the tritoxide (or gelatin) which is procured from the fibrin in largest quantities during inflammation; and, as Mulder's researches show that albumen is as easily resolved into this substance by boiling as fibrin itself, it seems to be a just conclusion that, during inflammation, this tritoxide is formed from albumen as well as fibrin.

All remedies which are intended to subdue inflammation must, therefore, act by tending to diminish the quantity or prevent the formation of the oxy-protein; and the known antiphlogistics undoubtedly act in this way. Blood-letting, by directly diminishing the quantity of the oxy-protein, acts as an antiphlogistic. Purgatives act in the same way, by increasing the secretions into the digestive tube.

M. Mulder thinks that the presence of these oxides of protein in arterial blood explains how the large amount of oxygen absorbed from the air may be disposed of—an amount quite disproportioned to that of the hæmatosin, supposed till now to be the sole oxygen carrier. These oxides of pro-to 0.715, or of carbon to 0.195. When on the butter diet for five days they yielded 0.623 of carbonic acid, or 0.169 of carbon.

M. Letellier's memoir is terminated by recording four experiments, from which he concludes that cane sugar is not deleterious as an aliment, excepting when given in enormous quantities; and that, even then, it is much less so than the sugar of milk.

From all his experiments he concludes, 1. That cane-sugar does not favour the production of fat; still less does the sugar of milk. 2. That butter, and probably also other fat substances, are not stored up in the tissues of the body when given as the sole aliment. 3. That an insufficient aliment prolongs life and diminishes the daily loss of weight of the body, provided it is not given in too great quantities.

Researches on Alimentary Substances. By MM. **Bernard** and **Barreswil**. (*Journal de Pharmacie*, June 1844.)—MM. Bernard and Barreswil ascertained that, by injecting various aliments dissolved in gastric fluid into the veins of animals, they could ascertain their comparative nutritive powers. If the substance was capable of being assimilated, they found that it disappeared entirely from the blood, and could not be detected in the secretions, but that if it was not assimilated it was passed off by the secretions, as by the urine. They found that if alimentary substances were injected into the veins without having previously undergone artificial digestion in gastric fluid, they were not assimilated, but were passed off in their original state by the urine. When alimentary substances were mixed with gastric fluid, and artificially digested for a few hours previous to being injected into the veins, they were found to be assimilated as perfectly and as completely as if they had been introduced into the stomach. The first series of the experiments are alone as yet published. They alone refer to sugar, albumen, and gelatine.

Aqueous solutions of sugar, of albumen, and of gelatine from isinglass were injected severally into the jugular veins of three dogs. After three hours the urine of each dog was examined. In that of the first dog the sugar was found unchanged; in that of the second, the albumen was quite distinct; and in that of the third, the ordinary reagents manifested the presence of gelatine.

Similar quantities of these three substances were then digested at a blood heat, for about six hours, with recent gastric fluid taken from the stomach of a dog, and were then severally injected into the veins of three dogs. The urine was drawn from the bladders, and examined after three hours. In the urine of the dog which got the sugar, no trace of sugar could be detected. In the urine of that one which got the albumen, no trace of albumen was perceived. But in that of the dog which had the gelatine, that substance was found present in appreciable quantity.

Three dogs were then fed for some time with these substances; one with sugar alone, the second with albumen alone, and the third with gelatine. The sugar and albumen could never be detected in the urine, but the gelatine was so invariably.

In order to test these last results still more accurately, MM. Bernard and Barreswil repeated them on themselves. After fasting they took sugar, but never could detect its presence in the urine. In a similar manner they took albumen, but it also could not be detected in the urine. When, however, they took gelatine, they always found that it was thrown off along with the urine.

May not this last fact explain why gelatine has been found experimentally not to contribute to nutrition?

Physiological Effects of Bathing in Cold Water. By Dr **Herpin** of Geneva. (*Gazette Medicale de Paris*, 20th April 1844.) Dr Herpin has made some very interesting inquiries as to the effects of cold bathing, which will serve to explain many of its therapeutic uses and counter-indications. His observations were made on bathers in the river Arne, a stream whose waters had an average summer temperature of 52° to 55° Fahrenheit. He found that, after the body was immersed in the water for about a minute, the pulse could not be perceived at the wrist, but that the pulsations of the heart became more and more intense in force in proportion to the length of the immersion, yet were not increased in frequency. In fact, in all his observations the number of pulsations of the heart while in the cold water remained the same as before the immersion. The cold-bath thus produced a marked diminution of the force of the arterial circulation in the extreme parts of the body, presenting a true obstacle to the circulation, which the heart, by its increased force, endeavoured to overcome. In the cold water the temperature of the body rapidly fell, so that when a thermometer was pressed between the thighs after immersion for even one minute in the water, it generally ranged from 71° to 77° Fahrenheit. When in the water the skin was in general pale, but assumed a more or less marbled or bloated bluish-red colour when the bathers returned from the water. The lips and mucous surfaces were at the same time observed to have a violet or livid colour. This redness in patches, and the violet colour of the mucous surfaces, appeared to be owing to a state of passive congestion of the blood in the capillaries, perhaps mainly attributable to the constriction and diminished circulation in the superficial veins. The breathing was at first convulsive or gasping, in some cases even amounting to a fit of dyspnœa, with difficulty of speaking while in the water. Dr Herpin does not state his views as to the cause of this phenomenon. These are the most important points in Dr Herpin's observations; the other effects,—the constriction of the skin, the firming of the muscles, the greater alacrity of body and mind, the increased feeling of strength, greater relish for food, &c.,—are too well known to require mention.

PATHOLOGY.

Analysis of the Blood in a case of Lead Colic. By Professor Cozzi. (*Journal de Pharmacie*, February 1844.)—Professor Cozzi, in analyzing the blood of a person severely affected with lead colic, discovered that the lead existed in the state of a salt or of an oxide of the metal. He at the same time ascertained that the lead was neither united with the hæmatosin nor fibrin, but was wholly contained in the albumen of the blood.

Analysis of the Blood of Persons exposed to Malaria. By M. Salvagnoli. (*Gazette Medicale de Paris*, 18th May 1844.)—M. Salvagnoli examined the blood of four persons actually labouring under, or who had just recovered from intermittent fever, and were living in a malarious district. He found that it contained a notable diminution in the proportion of its fibrin, albumen, and fatty matter, and that the phosphates had almost entirely disappeared. It contained, however, a large quantity of cholesterine. It is remarked that the biliary secretion of those living in such districts has been previously noticed to be rich in cholesterine.

Passage of Metallic Mercury into the Blood and Solid Tissues. By M. Œsterlen. (*L'Experience*, 1st August 1844.)—M. Œsterlen has made a number of experiments for the purpose of ascertaining whether metallic mercury, when rubbed on the skin and given internally in a state of fine division, enters the body in the metallic state. For this purpose he both rubbed mercurial ointment on the shaved skins of animals, and administered a certain quantity internally. In both cases he found globules of mercury from the 250th to the 1000th of a line in diameter in the blood. The mucous contents of the colon also contained distinct globules of mercury. Metallic globules were also detected in the mesenteric glands, in the liver, spleen, lungs, kidney, in the bile, and in the urine; they were also found in the cavities of the heart. The author also states that he has detected metallic globules in the saliva and urine of a woman who was salivated by mercury. From his experiments, M. Œsterlen concludes that in general small quantities of mercury, introduced by friction or by the stomach, are thrown out of the system by means of the urinary and biliary secretions.

Case of Obstruction of the Rima Glottidis by tumours. By Dr Corrigan. (*Dublin Journal of Medical Science*, March 1844.) A woman, 40 years of age, had suffered from chronic laryngitis for seven or eight weeks, during which time various medical means were employed without relief. She was now very weak, her respirations about thirty in the minute, and accompanied by a loud crowing sound similar to that of croup. The pulse was 120 and feeble. Nothing unnatural could be discerned in the throat, and pressing on the larynx caused no pain. There was no dulness over any part of the chest, nor any unusual stethoscopic phenomena, unless occasionally faint mucous and sonorous sounds. The croupy sounds of the inspirations were heard most distinct when the stethoscope was applied to the larynx. The patient died the next morning. On examination it was found that the *rima glottidis* was almost completely closed by two white firm mammillary bodies, situated immediately above the arytenoid cartilages, so that a small blow-pipe could barely be passed between them. It was quite apparent that very little air could hence pass through the glottis in this obstructed condition. The epiglottis was healthy, the mucous membrane of the larynx and trachea was red, and the larynx diminished in general capacity. The trachea and bronchial tubes, even to their most minute remifications, contained a frothy mucus. The lungs were congested. The *pia mater* was very vascular, and the surface of the brain congested. Dr Corrigan was of opinion that the disease had commenced as the *œdema glottidis* of Bayle, and that the swollen part had afterwards become solidified. The bodies which obstructed the *rima glottidis* were very dense, and resembled fibro-cartilage in structure.

Death from compression of the Vena Cava by an aneurism of the Aorta. By Dr Law. (*Dublin Journal of Medical Science*, March 1844.)—A man, 42 years of age, after having had a slight cough for four months, suddenly felt a fulness in the face, and great oppression in the præcordial region. When seen in the evening, œdema of the integuments over the thorax had taken place; and his face, neck, hands, and arms were of a remarkably deep livid colour. The superficial veins of the neck were hard and firm to the touch, and the whole of the superior parts of the body were in a state of remarkable venous congestion. There was considerable anxiety; the respiration was distinct; and there was no dulness over any part of the chest. The heart's action was rather diffuse, and audible under the clavicles. The intellects seemed unimpaired, though there was some slowness in answering questions. Blood-letting from the arm, and cupping the præcordial region, with the administration of stimulants, were had recourse to; but he got weaker and weaker, and sunk on the fourth day after the sudden accession of venous congestion. The heart was found completely concealed by the lungs; the opposite laminæ of the pericardium were adherent by effused blood; the *vena cava* was compressed and narrowed by means of an aneurism of the ascending aorta, which had pressed the sides of the vein together. The coats of the artery, at this point, were thin and transparent. Nothing was detected which could account for the suddenness of the compression.

On the Hereditary Transmission of Insanity. (By M. Baillarger. (*Gazette Medicale de Paris*, April 1844.)—M. Baillarger presented to the

Royal Academy of Medicine of Paris statistical tables, founded on 600 cases of insanity, traced to be connected with hereditary taint. His inquiries were directed to three points. 1. Whether insanity in the mother was more apt to be followed by insanity among the progeny than insanity in the father. 2. Whether a greater number of the progeny are attacked with insanity when the mother has been insane, or when the father has been insane. And, 3. Whether insanity of the mother is most frequently transmitted to the daughters, and insanity of the father to the sons. M. Baillarger ascertained that out of 453 cases of insanity, in which the disease had been transmitted by direct descent, the mother of the insane persons had been insane in 271 cases; the father, in 182 cases only. Thus showing that insanity was about a third more frequently hereditary when the mother was insane, than when the father was.

In answer to the second question, as to whether a greater number of the children become insane when insanity has been manifested by the mother or by the father, it was ascertained that of 271 families in which the disease was transmitted to the children by the mother, the disease attacked only 1 child in 203 of the families, 2 children in 62 families, 3 children in 5 families, and 4 children in 1 family,—making a total of 345 children attacked with insanity out of 271 families where the mother only was insane. It, on the other hand, appeared that of 182 families in which insanity appeared, in which the father alone was insane, 1 child was attacked with insanity in 152 of the families, 2 children in 26 families, and 3 children in 4 families,—making a total of 216 children attacked with insanity out of 182 families in which the father only was insane. From this it appears that in hereditary insanity the disease is transmitted to more than one child in one-fourth of the cases when the mother is insane, but only to one-sixth when the father is insane. Insanity on the mother's side is, therefore, not only more frequently transmitted to the offspring than when on the father's side, but also attacks a greater number of the children of the same family.

As to whether the insanity of the mother is more frequently transmitted to the girls, and that of the father to the boys, M. Baillarger found that of 346 insane children descended from insane mothers, 197 were girls, and 149 boys; the difference showing a fourth greater number of girls. Of 215 insane children descended from insane fathers, the number of boys was 128, that of girls 87 only; or a difference of a third in favour of the boys. Insanity in the mother appears, therefore, to be transmitted more frequently to the girls than to the boys in the proportion of a fourth; while insanity in the father is transmitted to the boys in the proportion of a third more than to the girls. Viewing the question in another light, allows other conclusions to be drawn. Of the total 331 insane boys, 149 inherited the disease from their mothers, 128 only from their fathers; showing a difference in favour of the hereditary descent of the disease from the mother in the proportion of a sixth. Of the total 274 insane girls, 187 inherited the disease from their mothers, and only 87 from their fathers; showing a difference in favour of the hereditary transmission of the disease from the mother in the proportion of more than a half.

On the transmission of Hydatids by Contagion. By Professor KLENCKE, (*Gazette Medicale*, December 1843.)—Hydatids have been usually regarded as beings developed spontaneously within our tissues, but it was reserved for Professor Klencke to demonstrate that they may be, and probably in every case are, developed by contagion as he calls it, or by actual introduction within the body. When he injected hydatids within the veins, placed them in the cellular tissue, or introduced them into the digestive tube of dogs, cats, birds, &c. he found that they excited the disease in these animals in a very short time.

In the course of his investigations he discovered that the milk of the cow frequently contained hydatids. He endeavoured, therefore, to ascertain whether these parasites, which are so frequently developed in man, could be attributed to the ingestion of aliments which contained them. The process of cooking he found destroyed these animals, but as many animal aliments, and notably milk, are taken in an uncooked state, he sought to ascertain whether they could pass unhurt through the digestive apparatus. To satisfy himself on this point, Professor Klencke macerated hydatids in the gastric juice of man and of dogs, and found that these singular animals were destroyed by this process. If milk and water, however, were mixed with the gastric juice in which they were macerated, that is to say, if they were placed as nearly as might be in the situation which they would occupy in the stomach during a repast, they continued to live, and were capable of communicating the disease.

These observations appear to Professor Klencke capable of explaining how these singular beings are introduced into the human body without any appreciable and tangible cause, seeing they are inclosed in our food in a microscopic state of minuteness. He found the hydatids of man easily communicable to the lower animals, and, by a parity of reasoning, thinks the hydatids of the lower animals may with equal facility be transmitted to man.

On the Causes of Albuminuria. By M. Fourcault. (*Comptes Rendus des Seances de l'Academie des Sciences,* 5th May 1844.)—From observing that suppression of the cutaneous transpirations was one of the fruitful sources of all diseases in all climates, and from finding that suppressing this same secretion in animals, by means of impermeable coverings, the same morbid phenomena were manifested, he was led to study what these diseases were. An alteration in the nature of the blood, sometimes dissolution of its organic elements, alterations of the secretions, deposits in various organs, local lesions, vascular engorgements like those which occur in tropical fevers, or in some of the severer ones of milder climates, were the usual symptoms produced by suppression of the cutaneous transpiration. When the suppression was sudden, the disease was more severe; when more slow, the malady assumed a chronic form.

With regard to albuminuria, M. Fourcault determined, by actual experiment, that the disease could easily be produced by suppressing the cutaneous transpiration. In dogs this disease was induced by covering the skin with an impermeable covering; and also by removing the skin, and then applying the impermeable covering.

Endeavouring to ascertain the material cause of this effect of the impermeable covering, M. Fourcault supposes that it is the excess of lactic acid which is then found in the blood, which reacts on the albumen, and gives rise to the production of albuminuria. Scrofula, tubercular deposits, caries, chronic thickening of the cellular tissue of infants, elephantiasis, lepra, phlegmasia dolens, &c. are all referred by the author to the same cause.

The following are some of the conclusions which he draws from all his observations. 1. The artificial suppression of the cutaneous exhalation gives rise to five orders of phenomena,—a complete alteration of the blood,—a marked lowering of the temperature,—increased secretions and effusions of different kinds,—local lesions and vascular engorgements,—alterations in the nature of the urinary secretion,—and lastly, albuminuria, which, however, can also, though very rarely, originate from a primary affection of the kidneys.

2. The introduction of the lactate of soda into the veins produces albuminuria, by favouring the excess of lactic acid in the blood.

3. When the acid secretion of the skin is suddenly stopped, it produces a marked change in the organic elements of the blood. This alteration is

remarked in Asiatic cholera, in plague, yellow fever, and in some of the severer diseases of temperate regions.

4. When this secretion is gradually suppressed, a number of chronic diseases are produced, among which is remarked albuminuria.

Researches on Albuminuria. By Dr Meyer of Tubingen. (*Archiv fur Physiologische Heilkunde*, January 1844.)—Dr Meyer, from researches into the cause of albuminuria, has arrived at the conclusion, that it may be produced by an accumulation of blood in the kidneys, without any organic alteration of the structure of these organs; and that this accumulation is produced either by too large a quantity being carried there by the arteries, or by a partial stagnation taking place in the veins. In this way he accounts for the occurrence of albumen in the urine, in diseases of the heart and lungs. These conclusions were supported by five direct experiments he made on animals. In some he diminished the calibre of the renal veins, or of the cava, by means of ligatures put loosely around the vessels; in others, he tied the abdominal aorta below the origin of the renal arteries. In all these cases the urine became albuminous.

Ascites, with Bright's disease of the Kidney after Scarlatina. By Mr O'Ferral. (*Dublin Journal of Medical Science*, March 1844.) A girl, shortly after scarlatina, became affected with symptoms of abdominal dropsy, and with anasarcous swellings confined to the lower extremities. There was great difficulty of breathing, and severe pain in the left hypochondrium, which caused the child to scream violently. The abdomen was greatly distended, and fluctuation quite distinct. Under these symptoms she gradually sunk, and for several hours before death lay completely comatose. The abdomen contained a large quantity of a straw-coloured fluid, which had so distended the peritoneum as to have displaced several of the viscera, having driven up the liver as high as the fourth rib, and very materially diminished the capacity of the chest. There were no lymphy deposits in any part of the abdomen. The liver was congested and its serous coat opaque. The lungs were so compressed that they resembled carnified lung, though they were not quite so solid. The most interesting circumstance was the existence of Bright's disease in the cortical part of the kidneys.

Wound of the Heart temporarily healed. By M. Marini. (*Il Raccogliatore Medico*, February 1844.)—A woman, 32 years of age, was stabbed with a dagger in the region of the heart, immediately fell, and lost a large quantity of blood. M. Marini, who saw her very shortly afterwards, found her just in life, covered with a cold clammy sweat, continually fainting, and with a wavering weak pulse. The wound was near the mamma, about two inches and a few lines from the edge of the sternum, between the fourth and fifth ribs. It was judged that the pericardium at least was injured, and that the wound was mortal. She was, therefore, carried to the hospital, and nothing was done till next day, when, as she was still alive, she was more carefully examined, and as the surgeons agreed that the wound was simply superficial and injured no important part, little was done, excepting drawing blood from the arm, or applying a few leeches to keep down threatened inflammation. Several times, however, she was in imminent danger of death. The external wound cicatrized, and she was dismissed as perfectly cured, after having been in the hospital about six weeks. Three weeks after this, when rising suddenly from her bed one morning, she fell down and instantly expired.

The external cicatrix was found solid and perfect. The condensed tissue, which filled the line of the original wound, could easily be traced between the fourth and fifth ribs into the interior of the chest. Half-a-pound of colourless serous fluid was found in the left pleura, and the upper lobe of the lung had contracted many firm recent adhesions. The part of the pericardium where it had been penetrated by the dagger was much thickened, and presented traces of acute inflammation. A large cyst, of a blackish-blue colour, filled with partly fluid, partly coagulated blood, adhered by a large pedicle to the left side of the pericardium. The pericardium was filled with about two pounds of blood, partly fluid, partly clotted; the heart was atrophied, its coats thinned, and its cavities full of blood. Near its apex, it was pierced with a rounded conical-shaped aperture, which communicated with the left ventricle. This aperture, which was so large as to permit the introduction of an ordinary pair of forceps, was surrounded by a ring of soft whitish lymph, which appeared to have adhered to the pericardium at the point penetrated by the dagger, and to have prevented the further effusion of blood.

On the formation of false Membranes within the Bladder from Cantharides Blisters applied to the Skin. By Dr Morel-Lavallee. (*L'Experience,* 18th July 1844.)—Dr Morel-Lavallee has recorded four cases of the formation of false membranes within the bladder in consequence of blisters of cantharides applied to the skin. In some individuals the tendency to the inflammatory affection of the bladder and the formation of false membranes was so great, that in the second case he records the smallest flying blister of cantharides, applied to the forehead, excited the inflammatory affection as often as it was applied. The symptoms attending the complaint are, frequent desire to micturate, pain at the *meatus urinarius* immediately after passing the last drops of water, the pain at length increasing so much that, when urine is passed, it gives rise to a burning sensation, as if the urine were melted lead. At length small soft pseudo-membranous pellets are expelled along with the urine. In the milder forms there is no fever, but in the more severe forms the fever is often intense, and the membranous substances obstruct the urethra, and are passed in long rolls; the urine is also highly albuminous. Copious emollient drinks, emollient cataplasms to the hypogastric region, or a hip-bath, and removal of the blister, in general suffice to cure this affection of the bladder.

MATERIA MEDICA AND THERAPEUTICS.

Latest Analysis of Cod-liver Oil. By M. Jongh. (*Journal de Pharmacie,* May 1844.)—M. Jongh has lately analysed the three varieties of liver oil procured from six different species of the genus Gadus. That, however, procured from the cod is said to be the most abundant and the best. Three kinds of oil are procured from the liver of these fishes. The first is clear and colourless, and is the first which separates from the livers when they are cut in fragments and suspended in bags; a brown oil then begins to drop away, and this forms the second variety; and the third is a very dark oil procured by boiling the livers in water, on the surface of which it floats.

Besides the principles of the bile, and various forms of the fatty acids, M. Jongh discovered that every variety of the cod-liver oil contained a considerable proportion of iodine, bromine, chlorine, phosphorus, and phosphoric acid. All these active principles exist in much larger quantity in the clear and in the brown-coloured oil, than in the dark variety. The actual proportions of these ingredients in the different varieties of oil was in 100,000 parts of the oil,

	Black oil.	Brown oil.	Clear oil.
Iodine, . .	29	40	37
Chlorine and a little bromine,	84	158	148
Phosphorus, .	7	11	21
Phosphoric acid, .	53	78	91

It is to the presence of the phosphorus and phosphoric acid, as much as to that of the iodine, that the therapeutic effects of these oils is attributed, and M. Gobley remarks, that in the case of rickets in particular, the curative effects of the oil ought to be attributed to them alone, seeing that iodine has no acknowledged efficacy in the removal of that complaint, while these supply one of the essential ingredients of the bone, which are defective in that disease.

On the Chemical and Therapeutic Effects of Calcined and Hydrated Magnesia. By M. MIALHE. (*Journal de Pharmacie,* June 1844.)—M. Mialhe shows that magnesia, like lime, exists in two distinct states,—in a caustic and in a hydrated state. When the caustic magnesia is mixed with ten times its weight of water it gradually absorbs the whole and becomes converted into a solid mass,—the hydrate of magnesia. So long as it is in a caustic state it has a far greater affinity for water than for any acid, so that even a stream of carbonic acid may be passed over it and yet none will be absorbed. From this circumstance it happens that, if kept in a dry place, it may be preserved unaltered for several years, as M. Mialhe, along with M. O. Henry, ascertained, and not have absorbed any appreciable quantity of carbonic acid during all that time. Even the hydrate of magnesia absorbs carbonic acid very slowly,—much more slowly than chemists imagine. Caustic magnesia alone answers as a test for the purity of balsam of copaiba. If any part of the magnesia has become hydrated, it exhibits the same action with the balsam as if the latter were adulterated. The French find that all the English calcined magnesia is an imperfect hydrate, containing a fifth part of its weight of water.

M. Mialhe concludes from his experiments that caustic magnesia ought never to be employed in medicine. In this state its causticity is extreme, and if laid on the hand burns it as rapidly as quicklime. When administered internally in the caustic state, it is with difficulty dissolved in the acids of the stomach, and is hence apt to cause concretions; besides, from its property of attracting water to form a hydrate, it absorbs the fluid of the stomach, dries the mucous membrane, and often occasions sensations of tenesmus, constriction, cardialgia, and thirst. Drinking plenty of water is the way to relieve these symptoms; but in very many cases this preparation requires to be abandoned, on account of the extreme uneasiness to which its administration gives rise. It is, therefore, recommended in every case to make use of the hydrated magnesia, to administer it suspended in ten or twelve drops of water, and drink a considerable quantity of water after it. M. Mialhe finds that, when taken in this way, two drachms form a safe and efficacious purgative within five or six hours after its administration. The English calcined magnesia, which contains a fifth of its weight of water, is reckoned the best preparation yet made; it is sufficiently hydrated to prevent any deleterious action. Caustic magnesia may easily be reduced to the state of a safe hydrate by exposing it to damp air for two or three days.

Iodine Injections in Ascites. By M. DIEULAFOY. (*Bulletin de l'Academie Royale de Medecine,* June 1844.) In a letter to the Academy of Medicine of Paris, M. Dieulafoy states, that in the only two cases of ascites in which he made trial of iodine injections he succeeded in procuring a radical cure. The first trial was made three years ago, and the disease has not since returned; the second was under treatment the time the letter was written. A young girl had been bedridden from abdominal dropsy for five months, during which time the fluid had been evacuated by puncture five different times. It always rapidly reformed, requiring to be again drawn off. After the fluid was last drawn off, an iodine solution was injected within the peritoneal cavity. No bad symptom followed. Twenty days after the peritoneal cavity appeared obliterated, no fluid had been effused, the abdomen was flat, the girl had left her bed and was able to walk freely about.

Nervous Aphonia cured by Electricity. By M. Pellegrini. (*Journal de Pharmacie*, January 1844.)—A healthy man, 23 years of age, was imprisoned for having killed a friend in a quarrel. Three days afterwards he had a severe fit of epilepsy, and lost his voice. Active treatment had no effect on the aphonia, though it removed the epileptic tendency. The larynx seemed to have lost the power of motion; the tongue was moved with difficulty, was somewhat swollen, and drier than natural. Sixteen months after the attack electricity was employed. A voltaic pile of fifty plates was used, the zinc wire of which was applied to the region of the first vertebra, the copper wire to the tongue and glottis. One hundred shocks were passed through these parts the first day, and two hundred the second. On its repetition an epileptic fit and syncope occurred, for which he was bled. When he recovered he was able to speak indistinctly in a hoarse voice. After eight sittings, during which the same number of shocks were given each time, the voice was completely restored, and the cure proved radical and complete.

Valerianate of Zinc, a powerful antispasmodic. By M. Devay. (*Gazette Medicale de Paris*, 29th June 1844.)—The valerianate of zinc has lately been highly commended in several Italian Journals as a valuable therapeutic agent in the cure of neuralgic affections. M. Devay having made numerous trials of its powers in these affections, arrived at the conclusion, that it was a remedy of no mean powers, which deserved to be more generally used. It appears, however, from the cases which he records, that its curative powers are confined to those in which the disease depends on a purely nervous affection, and in the excitable nervous affections which accompany chlorosis, in facial neuralgia, or tic-douloureux, not depending on an organic cause, in hemicrania, and even in some cases of satyriasis and epilepsy, its therapeutic virtues have been powerfully manifested. Several cases are recorded by M. Devay of its curative powers. It seems to act as a powerful antispasmodic, combining the powers of valerian and of zinc. The dose is half a grain two to four times daily.

The valerianate is prepared in the following manner. The fresh roots of valerian are distilled, when the valerianic acid comes over along with the essential oil. This oil is separated, and the distilled water has its acid saturated by carbonate of potash. Solution of caustic potash is also agitated with the essential oil, and both fluids are mixed together. The valerianate of potash not being volatile, allows the most of the water to be driven off, as well as that portion of the volatile oil which has not united with the alkali. When the valerianate of potash is sufficiently concentrated, it is introduced into a small retort, and a sufficient quantity of dilute sulphuric added to unite with the potash. Heat is then carefully applied, and the volatile valerianic acid distils over in a pure state, partly dissolved in a small quantity of water, partly as an oily hydrate. It is then mixed with carbonate of zinc, and the union aided by heat. It is then filtered, and as the fluid cools the crystals of the valerianate of zinc are deposited. The mother liquor is to be evaporated till all the salt is obtained.

On the treatment of Syphilis by Tartar-Emetic. By Dr Willebrand of Finland. (*Gazette Medicale de Paris*, 6th April 1844.)—Dr Willebrand was induced to make trial of tartar-emetic in the treatment of syphilitic complaints, in the military hospitals, from observing the rapid removal of a blenorrhœa in a patient who was using that medicine for an attack of rheumatism. On trying the efficacy of this medicine on a large scale, he found that urethral discharges were, in general, removed in from six to fifteen or, at the most, twenty days. It was, however, in the cure of true syphilis that this medicine was found of superior efficacy. Cases of primitive chancre were cured by the internal use of tartar-emetic alone, in from ten to twenty days, no application having been made to the sore but simple water dressing. In a few cases, in which there was much induration of the base of the sore, a cure was not effected. In thirty cases of secondary affection, under the form of ulcerations of the throat, &c. all symptoms of the disease disappeared in from eleven to fifteen days. The tartar-emetic was, however, continued, as a precautionary measure, for five or six days longer, after which the medicine was discontinued; and though two, and, in some instances, three years have since elapsed, no relapse has occurred among all these cases. Most of the cutaneous affections rapidly and easily yielded; but some of the more inveterate ones, as the pustular and squamous syphilitic eruptions, required from twenty to twenty-eight days for their removal. In some of these, from the antimony disagreeing, it was requisite to intermit its use.

In all the cases, the tartar-emetic was administered internally in the same manner, viz. half a grain six or eight times a-day. The first doses generally produced vomiting; but by the second day tolerance was produced. When this large dose seemed to disagree, an eighth of a grain was given in the same manner, but then it often failed to effect a permanent cure. Cleanliness, repose, an equable temperature, and regulated diet were the only adjuvant means used; and Dr Willebrand thinks that these cases show, that, in very many cases, the tartar-emetic may be substituted with advantage for the more dangerous mercurial treatment.

Sublimed Sulphur a vermifuge in Tenia. By Dr Tridenti. (*L'Experience*, September 5, 1844.) Dr Tridenti having met with fifteen cases of patients afflicted with tenia with whom the pomegranate root bark disagreed, was induced to try the efficacy of sublimed sulphur. He was led to try sulphur, from one of the patients requiring to be treated for an hepatic eruption, for which sulphur, in the dose of two drachms, was daily administered. On the eighth day he was astonished to find that half of the tenia was passed, and the other half came away on administering a dose of decoction of pomegranate root bark. In these fifteen cases sulphur was alone administered, and produced the expulsion of the tenia. It was given in doses of two drachms daily, and the dose was repeated in the course of the day, when the uneasy intestinal movements were experienced which generally precede the expulsion of the worm.

Case of recovery from wound with Hernia of the Lung. By Dr Barbieri. (*L'Experience*, August 22, 1844.) A boy, 13 years of age, fell from a tree upon the point of a hay fork, which tore up the right side of the anterior part of the chest between the fifth and sixth ribs. The wound was transverse, about three inches in extent, and through this projected a considerable piece of the lung, also wounded in a transverse direction. There was a frequent cough with a gurgling sound in the bronchial tubes, but no bloody expectoration. The usual phenomena of a severe accident were present. The lung was reduced, the lips of the wound brought together by suture, and, by following an antiphlogistic regimen, the patient recovered without a bad symptom.

OBSTETRICS.

Ligature of one of the Heads of a Bicephalous Fœtus. By Dr Buhring. (*Casper's Wochenschrift fur die Gesammte Heilkunde*, March 1844.)—On the 31st of October 1843, Dr Buhring was called by a peasant of the village of Kleinow to remove a tumour from the head of his child, just born.

He found the infant was eighteen hours old, and had a large shapeless tumour, covered with hair, attached to the posterior part of its head. On this tumour were traces of a nose, with depressions for the eyes and mouth, a kind of elastic semi-cartilage instead of bone, and a regluar hairy scalp. At the posterior part of this evidently supernumerary, but ill developed head, there projected a small red tumour, with distinct fluctuation. Dr Buhring made an incision into this small fluctuating tumour, when a quantity of clear serous fluid escaped; and the wound being enlarged, allowed him to discover that the ill-developed supernumerary head contained a small brain with two distinct hemispheres, the convolutions of which were distinctly marked, and which were separated from each other by a falciform process. Notwithstanding this discovery, he determined to remove this supernumerary head by ligature, as it seemed only to be connected with the proper head by soft parts. A strong waxed ligature was, therefore, passed around it; and it was observed that, in proportion as it was tightened, the respiration, previously easy, became painful and hurried, the pulse more frequent, the pupils of the eyes dilated, the vessels of the true face and scalp strongly injected with blood, and the jugular vein projected as a tense cord. The jugular vein was, therefore, opened before the ligature was drawn to its full tightness. About three ounces of blood were allowed to flow, after which all the disagreeable symptoms disappeared, except a slight difficulty of breathing, and some slight irregular (convulsive ?) movements in the limbs. The child after this freely took the breast, and remained quiet, and fell asleep for a couple of hours after the operation, after which Dr Buhring left; but when he returned next day the child was dead. The supernumerary head did not communicate with the other by medullary matter, but only by means of nerves, blood-vessels, and a prolongation of the *dura mater*. The brain consisted of two hemispheres, a *pons varoli*, and the commissure; the *crura cerebri* and all the other parts at the base of the brain were wanting. It was covered by the usual membranous envelopes.

TOXICOLOGY.

Nine persons poisoned by roots of Henbane. (*Encyclographie Medicale*, March 1844.)—A woman collected in a fallow field a quantity of the roots of hyosciamus in mistake for parsneps, which she knew had been sown there. They were boiled in soup along with other pot-herbs, and the soup and vegetables were pronounced excellent by all the family. Nine persons partook of the soup, and very shortly afterwards they felt extremely uneasy, experienced an acrid and bitter taste which excited nausea. Their pupils became dilated and they could not see distinctly; their extreme uneasiness prevented them from remaining in one place, and they were shortly afterwards attacked with convulsions and continued delirium. They successively lost the power of vision, of hearing, and of voice, and fell into a state of prostration, with insurmountable stupor and somnolency. Emetics and a stimulant plan of treatment being immediately had recourse to, saved the lives of all.

Fatal case of poisoning by Cantharides. By Dr Bonacossa. (*Giornale delle Scienze Mediche di Torino*, June 1844.) A man, 46 years of age, labouring under paralysis and insanity, swallowed by mistake about a half-ounce of cantharides plaster, containing scarcely two drachms of the powder of cantharides. Within a quarter of an hour his mistake was discovered, and an emetic being administered, the greater portion was ejected, enveloped in much glairy mucus. Mucilaginous drinks were then administered. Within two hours and a half, his lips, and the whole mucous membrane of the mouth, were red, and covered with small blisters. Seven hours after this coldness of the body came on, and the pulse could scarcely be felt. He recovered somewhat, but sunk, with all the symptoms of exhaustion, 24 hours after swallowing the cantharides. No erection of the penis was observed during life; but it was livid, and the urine which was passed was mixed with blood.

Besides marks of ancient lesions in the brain, its vessels were found gorged with blood, the ventricles and between the convolutions filled with serous fluid. The cerebral substance was injected and softened. The heart and lungs were healthy. The internal surface of the stomach was covered with red points, interspersed here and there with ecchymosis, in the centre of which was seen an adhering particle of the cantharides powder. The intestines appeared healthy. The kidneys were very red and gorged with blood, and slightly granular. The left ureter was internally of a very red colour. The bladder was much thickened, and its mucous coat injected with blood.

Case of poisoning by Cantharides. By Dr PODRECCA. (*Annali Universali di Medicina*, November 1843.)—A man 30 years of age swallowed 20 grains of cantharides, which his companions had put in his food for a joke. During the night general uneasiness came on, with a sensation of heat and constriction in the throat, nausea, efforts to vomit, shiverings, and pains in his limbs. The pains soon extended to his back and loins, and he was seized with tenesmus, difficulty in making his water, yet constant desire to micturate, pains in the bladder and urethra, constant tendency to syncope, and intolerable thirst. The next day the symptoms were still more alarming, and at length paralysis of the lower extremities and micturition of blood were observed It was not till then that medical assistance was called, when, in addition to these symptoms, it was found that his strength was completely gone, his face was livid, his eyes surrounded with a black circle, his whole body was cold and covered with a clammy sweat, and he had priapism at intervals. Under the stimulus of wine, bark, and laudanum, he recovered.

MEDICAL POLICE.

A Bill for the better Regulation of Medical Practice throughout the United Kingdom. [Note.—The words printed in *Italics* are proposed to be inserted in the Committee.]—1. Whereas it is for the good of all Her Majesty's subjects that the knowledge of physic and surgery should be promoted, and that means should be afforded whereby those who have been examined and found skilful by competent authority, may be known from ignorant and unskilful pretenders to the same knowledge: And whereas the laws now in force concerning the profession of physic and surgery require to be amended; Be it enacted, by the Queen's most Excellent Majesty, by and with the advice and consent of the Lords Spiritual and Temporal, and Commons, in this present Parliament assembled, and by the authority of the same, That an Act passed in the third year of the reign of King Henry the Eighth, intituled, "An Act for the appointing of Physicians and Surgeons;" and also another Act passed in the fifth year of the same reign, intituled, "An Act concerning Surgeons to be discharged of Quests and other things;" and also another Act passed in the session of Parliament holden in the fourteenth and fifteenth years of the same reign, intituled, "The Privileges and Authority of Physicians in London;" and also two Acts passed in the thirty-second year of the same reign, respectively intituled, "For Physicians and their Privileges," and "For Barbers and Surgeons;" and also another Act passed in the Session of Parliament, holden in the thirty-third and thirty-fourth years of the same reign, intituled, "A Bill that persons being no common Surgeons, may minister Medicines, notwithstanding the Statute;" and another Act passed in the first year of the reign of Queen Mary, intituled, "An Act touching the Corporation of Physicians in London;" and also an Act passed in the Session of Parliament, holden in the sixth and seventh years of the reign of King William the Third, intituled, "An Act for exempting Apothecaries from serving the offices of constable, scavenger, and other parish and ward offices, and from serving on juries;" and so much of every other Act as continues the last-recited Act; and also an Act passed in the tenth year of the reign of King George the First, intituled, "An Act for the better viewing, searching, and examining of all drugs, medicines, waters, oils, compositions, used or to be used for medicines, in all places where the same shall be exposed for sale, or kept for that purpose, within the city of London and suburbs thereof, or within seven miles circuit of the same city;" and so much of another Act passed in the eighteenth year of the reign of King George the Second, intituled, "An Act for making the Surgeons of London and the Barbers of London two separate and distinct Corporations," as does not relate to the separation of the said corporations, or to the master, governors, and commonalty of the mystery of barbers of London; and also so much of an Act passed in the fifty-fifth year of the reign of King George the Third, intituled, "An Act for better regulating the practice of Apothecaries throughout England and Wales," as relates to the examination of apothecaries, or to the qualifications of persons intending to be examined, or to qualify themselves under that Act to practise as an apothecary, or to the fees to be paid by apothecaries for the certificate of the Court of Examiners, or to the penalties for practising as an apothecary without having obtained such certificate; and also so much of an Act passed in the sixth year of the reign of King George the Fourth, as enacts, that all members and licentiates of the Royal College of Physicians in London actually practising; all surgeons being members of the Royal Colleges of Surgeons in London, Edinburgh, or Dublin, and actually practising; all apothecaries certified by the Court of Examiners to the Apothecaries' Company, and actually practising, shall be freed and exempt from being returned, and from serving upon any juries or inquests whatsoever, and shall not be inserted in the lists to be prepared by virtue of that Act; and also so much of any Act or Charter granted before the *passing of this Act* as prohibits any person from practising physic or surgery in any place without such license as is mentioned in such Act or Charter respectively, or as imposes any restriction on the practice of physic or surgery other than is contained in this Act, shall be repealed and annulled.

2. And be it enacted, That a Council shall be established, which shall be styled "The Council of Health and Medical Education;" and that one of Her Majesty's Principal Secretaries of State shall be a member of the said Council, in right of his office as Secretary of State; and that the Regius Professor of Medicine in the University of Oxford, the Regius Professor of Physic in the University of Cambridge, the Regius Professor of Physic in the University of Dublin, the Regius Professor of Clinical Surgery in the University of Edinburgh, and the Regius Professor of Surgery in the University of Glasgow, shall be members of the said Council in right of their several professorships; and that the other members of the said Council shall be *one* physician and *one* surgeon, to be chosen by the Colleges of Physicians and Surgeons of England respectively; *one* physician and *one* surgeon to be chosen by the Colleges of Physicians and Surgeons of Scotland respectively; *one* physician and *one* surgeon to be chosen by the Colleges of Physicians and Surgeons of Ireland respectively; and *six* other persons whom Her Majesty, with the advice of Her Privy Council, shall deem fit to be members of the said Council.

3. Provided always, and be it enacted, That it shall be lawful for Her Majesty, with the advice of Her Privy Council, to appoint all the members of the first Council of Health and Medical Education, other than those who will be members thereof in right of their several offices; and that at the

end of the *third* and each of the *two* next following years after the first constitution of the said Council, *one* physician and *one* surgeon of those first appointed on behalf of the said several Colleges of Physicians and Surgeons, shall go out of office, in such order as Her Majesty, with the advice of Her Privy Council, shall direct.

4. And be it enacted, That those members of the said Council who are members in right of their several offices shall continue to be members thereof so long as they hold the same offices respectively, and no longer, and the *six* members of the said Council, appointed as aforesaid by Her Majesty, with the advice of Her Privy Council, shall continue to be members of the said Council during Her Majesty's pleasure, and upon every vacancy among the last-mentioned members of the said Council, and their successors, it shall be lawful for Her Majesty, with the advice of Her Privy Council, to appoint another fit person to be a member of the said Council during Her Majesty's pleasure.

5. And be it enacted, That upon every vacancy among the members of the said Council appointed on behalf of the said several Colleges of Physicians or Surgeons, and their successors, the Royal College of Physicians or Surgeons of England, Scotland, or Ireland, as the case may be, shall appoint another physician or surgeon, as the case may be, to supply such vacancy, subject to the approval of Her Majesty; and every member of the Council so chosen shall be entitled to be a member of the said Council for *three* years, and shall then go out of office, but may forthwith be re-chosen, subject to Her Majesty's approval: Provided always, That no president, vice-president or examiner of any of the said Colleges shall be qualified to be so appointed.

6. And be it enacted, That the manner of choosing the last-mentioned members of the said Council shall be determined in each case by orders or bye-laws to be made from time to time by the said several Colleges, subject to the approval of the Council of Health and Medical Education.

7. Provided always, and be it enacted, That it shall be lawful for any memmer of the said Council appointed by Her Majesty, or chosen by any of the said Colleges, at any time to resign his office, or for Her Majesty at any time, with the advice of Her Privy Council, to dismiss any such member of the said Council for notorious misbehaviour or unfitness, and upon any vacancy in the said Council by death, resignation or dismissal, another member of the Council shall be appointed in the same manner and for the same term as the member by whom the vacancy shall have been made.

8. And be it enacted, That it shall be lawful for the said Council, with the approval of one of Her Majesty's Principal Secretaries of State, to appoint a principal secretary for the said Council, and also local secretaries for Scotland and Ireland, and so many clerks and messengers, as the said Secretary of State shall deem necessary, and also with the like approval to remove at their pleasure any of the said secretaries, clerks and messengers, and appoint others in their room.

9. And be it enacted, That there shall be paid to the members of the said Council, and to the said secretaries, clerks and messengers, such salaries as shall be from time to time allowed by the Lord High Treasurer or Commissioners of Her Majesty's Treasury, who may also allow such reasonable travelling expenses which may have been incurred by any member of the said Council, or any secretary, clerk or messenger in the performance of his duties under this Act, and such other reasonable expenses of putting this Act into execution, as the said Lord High Treasurer or Commissioners of Her Majesty's Treasury shall think fit.

10. And be it enacted, That the said Secretary of State shall be president of the said Council, and shall be empowered from time to time to nominate one of the members of the Council appointed as aforesaid by Her Majesty

to be vice-president of the Council, and to act as president in his absence; and at every meeting of the Council in the absence of the president and vice-president, some other member to be chosen by the Council from the members then present shall be empowered to act as president.

11. And be it enacted, That the said Council shall be holden in such places and at such times as the Secretary of State from time to time shall appoint; and that all acts of the Council shall be decided by the votes of the majority of the members present at any meeting, the whole number not being less than *seven*; and at all such meetings the president for the time being shall have a second or casting vote in all cases of equality of votes.

12. And be it enacted, That minutes of the proceedings at all meetings of the Council shall be drawn up and fairly entered in books, to be kept for that purpose; and such minutes shall be at all reasonable times open to the inspection of any person or committee appointed for the purpose of inspecting them by any of the said Universities or Colleges.

13. And be it enacted, That a register shall be kept and published from time to time, under the direction of the said Council, of all persons who shall have been examined, and shall have received, and shall exhibit before the said Council letters testimonial, as hereinafter menioned, of their qualification to practise as a physician, or as a surgeon, or as a licentiate in medicine and surgery; for which registry the Council shall be entitled to have from the person requiring to be registered a fee of L.5 in the case of a physician or surgeon, and a fee of L. 2 in the case of a licentiate, which fees shall be applied toward defraying the expenses of this Act; and every person whose name shall be so registered, who shall be desirous that his name shall be continued in the published Register, shall, in the month of *January* in every year, send to the said Council his name and place of abode, with the date of his testimonials, and the Council shall verify the returns so made to them by comparison with the Register kept by them, and shall forthwith cause the names of all persons duly registered, and so returned to them, to be published in alphabetical order in their several classes, with their several places of abode, and dates of their testimonials.

14. And be it enacted, That no person, except such graduates in medicine and such other legal practitioners as are hereinafter mentioned, shall be entitled to be registered by the Council as a licentiate in medicine and surgery unless he shall have attained the age of *twenty-one* years, and shall have been examined by the Colleges hereinafter named; (that is to say,) if in England, examined by the Royal College of Physicians of England, assisted by the Court of Examiners of the Apothecaries' Company, and also examined by the Royal College of Surgeons of England; and if in Scotland, examined by the Royal Colleges of Physicians and Surgeons of Scotland; and if in Ireland, examined by the Royal Colleges of Physicians and Surgeons in Ireland; and in every case shall have received letters testimonial, from each of the bodies by which he shall have been examined, of his being duly qualified to practise as such licentiate.

15. And be it enacted, That no person, except such legal practitioners as are hereinafter mentioned, shall be entitled to be registered by the Council as a surgeon unless he shall have attained the age of *twenty-five* years, and shall have been examined by one of the Royal Colleges of Surgeons of England, Scotland, or Ireland, or the Royal College of Physicians and Surgeons of Glasgow, after such proofs as shall be satisfactory to the examining College that he has applied himself to surgical studies during at least *five* years; and shall have received letters testimonial from the examining College of his being duly qualified to practise as a surgeon.

16. And be it enacted, That no person except such legal practitioners as are hereafter mentioned, shall be entitled to be registered by the Council as a physician unless he shall have attained the age of *twenty-six* years, and shall have graduated in medicine in some university of the United

Kingdom of Great Britain and Ireland, or, subject to the restriction hereinafter contained, in some foreign university, and shall also have been examined by one of the Royal Colleges of Physicians of England, Scotland, or Ireland, or by the Royal College of Physicians and Surgeons of Glasgow, after such proof as shall be satisfactory to the examining college that he has applied himself to medical studies at least *five* years, or if he is not a graduate in medicine of any such university, unless he shall have attained the age of *forty* years, and shall have been examined by the Royal College of Physicians of England; and in each case shall have received letters testimonial from the examining college of his being duly qualified to practise as physician; and no person shall be entitled to be received for examination for the purpose of being so registered as a physician upon a foreign degree in medicine, unless the Royal College of Physicians of England, Scotland, or Ireland shall give him a special certificate, to be laid before and approved by the Council of Health and Medical Education, that they have made inquiry into the manner in which such degree was conferred, and have ascertained that it has been granted after residence within the precincts of the same university during at least *one year*, and after due examination, and upon satisfactory certificates of previous study.

17. And be it enacted, That it shall be lawful for the same person, if possessed of the necessary testimonials, to be registered as both physician and surgeon, and for a registered physician, or a person applying to be registered as a physician, to offer himself for examination as a licentiate in surgery by one of the said Royal Colleges of Surgeons, and for a registered surgeon, or a person applying to be registered as a surgeon, to offer himself for examination as a licentiate in medicine by one of the said Royal Colleges of Physicians, assisted in England by the Court of Examiners of the Apothecaries' Company; and every such physician or surgeon shall be entitled to be also registered upon the testimonials granted to him upon such additional examination, in such form and manner as shall be determined by the said Council.

18. And be it enacted, That every person registered after examination as a physician or surgeon under this Act, shall be admitted as an Associate of the Royal College of Physicians, or as a Fellow of the Royal College of Surgeons from which he shall have received his letters testimonial as physician or surgeon, or if he shall have received the said testimonials from the Royal College of Physicians and Surgeons of Glasgow, then as a Fellow of the last-mentioned Royal College; and every such physician and surgeon who shall afterwards remove from that part of the United Kingdom in which he obtained his letters testimonial, shall be required, if he shall practise as a physician or surgeon in any other part of the said United Kingdom, to enrol himself as an Associate of the Royal College of Physicians, or as a Fellow of the Royal College of Surgeons of that part of the United Kingdom to which he shall so remove, for the purpose of practising there, according to the nature of his testimonials, and in each case shall be entitled to be so admitted without further examination, and on payment of the like fees of admission, and on complying with the same conditions as are required of other persons who have passed their examinations for the purpose of being admitted associates or fellows of the said colleges respectively.

19. And be it enacted, That the said several colleges shall, from time to time, when required by the said Council, prepare and lay before the said Council a scheme or schemes of the course of study and particulars of the examination to be gone through by all persons applying to such colleges respectively for letters testimonial as physician, or surgeon, or licentiate, and of the fees to be taken for examination and admission into the said several colleges respectively; and the said Council shall be empowered to make from time to time such changes in any of the schemes so laid before them as to the said Council shall seem expedient; and the said Council

shall endeavour to procure, as far as is practicable and convenient, that the qualifications and fees for the said testimonials shall be uniform, according to the nature thereof throughout the said United Kingdom.

20. And be it enacted, That after the *passing of this Act* it shall not be lawful for any university of the said United Kingdom to confer any degree in the faculty of medicine upon any person, unless he shall have been matriculated in the same university, and shall have duly attended the courses of public lectures prescribed by the same university to students in medicine within the precincts of the same university, or of some medical school recognised by and in connexion with the same university, during at least *two* years next before the granting of such degree, and shall have been examined at some time before granting such degree by the proper examiners of such university, and found by them to possess competent skill and knowledge of medicine, and of the sciences connected therewith, and of the English and Latin languages at least; and every diploma or certificate of a medical degree granted by any such university after the *passing of this Act*, shall set forth distinctly the time which has elapsed since the matriculation of the person to whom such degree shall be granted, and the time during which, and place at which he shall have actually studied as aforesaid, and the fact that he has passed such examination as last aforesaid.

21. And be it declared and enacted, That it shall be lawful for any university of the said United Kingdom to grant the degree of Bachelor in the Faculty of Medicine, subject to the restrictions hereinbefore contained concerning medical degrees, to any student of the same university who shall have attained the age of *twenty-two* years; and that every such Graduate in the Faculty of Medicine, being also examined, and having received letters testimonial of his qualification in the manner hereinbefore prescribed in the case of licentiates in medicine and surgery, or examined and furnished with the like letters testimonial by the Royal College of Physicians and Surgeons of Glasgow, if the said degree shall have been granted by the University of Glasgow, shall be entitled to be registered by the said Council of Health and Medical Education as a licentiate in medicine and surgery, subject to such general regulations as shall be made by the said Council concerning the registry of licentiates.

22. And be it enacted, That no bye-law to be made by any of the Royal Colleges of Physicians or Surgeons of England, Scotland, or Ireland respectively, or by the Royal College of Physicians and Surgeons of Glasgow, shall be of any force until a copy thereof, sealed with the seal of the same college, shall have been laid before and approved by the said Council of Health and Medical Education.

23. And be it enacted, That it shall be lawful for the said Council to make registrations for insuring the registry of all medical and surgical students by the proper officers of the several hospitals or medical or surgical schools at which they shall study, and to authorise such officers to take a fee for such registration, not being more in each case than *ten shillings*, and for requiring all such fees to be remitted to the secretary of the said Council, and returns to be made to them of the registration of all such students, in such manner and form as the Council shall think fit; and no hospital or medical or surgical schools shall be recognised by any of the said colleges which shall neglect or refuse to give due effect to such regulations, after notice of such neglect or refusal shall have been sent by the said Council to the said colleges, until the default of such hospital or medical or surgical school be amended to the satisfaction of the said Council, and all such fees shall be applied toward the expenses of this Act.

24. And be it enacted, That where by this Act it is provided that the concurrence of more than one body is required for qualifying any person to to be registered by the said Council, the examination before such bodies

for his degree or letters testimonial, or both, may be conducted either separately before examiners appointed by each body, or before a joint Board of Examiners, to be appointed by each body separately or conjointly, who shall be appointed in such number, manner and form, and shall hold their examinations at such times and places as such bodies shall, with the approval of the said Council, agree from time to time among themselves, or as shall be determined by the said Council with respect to any point in which they shall not be agreed; and where there shall be separate examinations before examiners appointed by each body, the subjects and fees of examination shall be divided among such bodies as they shall from time to time agree among themselves, or as the said Council from time to time shall determine with respect to any subject on which they shall not be agreed.

25. And be it enacted, That the said Council may from time to time require returns to be made in such form, and including such particulars, as they shall think fit, respecting the examinations to be conducted as aforesaid, and it shall be lawful for any secretary of the said Council, deputed by the Council for that purpose, or for any member of the said Council, to be present at any of the said examinations; and if the Council shall be of opinion that the regulations prescribed by them for the examination and grant of letters testimonial as physician, surgeon or licentiate, have been infringed, evaded or neglected by any of the said examining bodies, it shall be lawful for the said Council to refuse to register upon the testimonials of the body so in default, until the same be amended to the satisfaction of the said Council.

26. And be it enacted, That, subject to the reservations hereinafter contained, no person after the *passing of this Act* who is not registered by the said Council shall be appointed to any medical or surgical office in any public hospital, prison, infirmary, dispensary, workhouse, or other public institution in the said United Kingdom, or to any medical or surgical office in Her Majesty's army or navy, or in the service of the Honourable East India Company, except in India, natives of India duly qualified according to such laws or regulations as are or shall be made in that behalf by the Governor-General in Council; and wherever by law it is provided that any act shall be done by a physician or surgeon, or medical or surgical practitioner, by whatever name or title he is called, such provision shall be construed, after the *passing of this Act,* to mean a person qualified to be appointed to such medical or surgical offices as aforesaid; and the Council of Health shall be empowered from time to time to make regulations for specifying what institutions are to be considered public institutions within the meaning of this Act, and which form of testimonial shall be necessary to qualify the holder thereof for every such situation.

27. And be it enacted, That all persons who are registered by the said Council as physicians, surgeons or licentiates, shall be exempt while practising as such from being summoned or serving on all juries and inquests whatsoever, and from serving all corporate, parochial, ward, hundred and township offices, but, subject to the reservations hereinafter contained, no person shall be entitled to such exemption, on the ground of his practising medicine or surgery, who is not so registered, nor shall the certificate of any such unregistered person, given after the *passing of this Act,* be received as the certificate of a medical or surgical practitioner in any court of law, or in any case in which by law the certificate of a medical or surgical practitioner is required.

28. Provided always, and be it enacted, That it shall be lawful for the said Council, on the application, within *twelve* calendar months after the *passing of this Act,* of any person legally practising as a physician, surgeon or apothecary, at the time of the *passing of this Act,* in any part of the United Kingdom of Great Britain and Ireland, or on the application within

two years of any person so legally practising in any of Her Majesty's colonies and foreign possessions to cause the name of such person to be registered as a physician, surgeon or licentiate in medicine or surgery, as the case may be, on production to the said Council of his diploma, license, or certificate, or such other proof as shall be satisfactory to the said Council, that at the time of the *passing of this Act* he was legally entitled to practise as a physician, surgeon or apothecary, as the case may be, in some part of the said United Kingdom, and on payment of a fee of *two pounds* in the case of fellows or associates of the said colleges of physicians and surgeons respectively, and of *five shillings* in every other case, which fees shall be applied toward the expenses of this Act; and during the said period of *twelve* calendar months every person legally practising as a physician, surgeon or apothecary at the time of the *passing of this Act* in the said United Kingdom, and during the said period of *two* years, every person so legally practising in any of Her Majesty's colonies and foreign possessions, although not registered, shall continue to enjoy the same privileges and exemptions, and be qualified to be appointed to the same offices, and to practise in the same manner as if this Act had not been passed, and no farther or otherwise, unless registered under this Act.

29. And be it enacted, That every person appointed after the *passing of this Act* to any medical or surgical office for which he is not qualified according to the provisions of this Act and the regulations of the said Council, and who shall act or practise in such office, shall for every such offence forfeit the sum of *twenty pounds*, to be recovered by action of debt or information to be brought in any of Her Majesty's Courts of Record at Westminster, or in the Court of Exchequer in Scotland, or in Dublin, within *six calendar months* next after the commission of the offence, and to be recovered in the name of Her Majesty's Attorney-General in England or Ireland, or of the Lord Advocate in Scotland.

30. And be it enacted, That after the *passing of this Act*, no person shall be entitled to recover any charge in any court of law for any medical or surgical advice, attendance or operation, or for any medicine prescribed or administered, unless he shall prove upon the trial either that he is registered under this Act, or that he was legally practising in the capacity in which he claims such charge before the passing of this Act.

31. And be it enacted, That every unregistered person who shall wilfully and falsely pretend to be, or take or use any name or title implying that he is registered under this Act, shall be deemed guilty of a misdemeanour in England and Ireland, and in Scotland of a crime and offence, and being convicted thereof, shall be punished by fine or imprisonment, or both, as the Court before which he shall be convicted shall award.

32. And be it enacted, That this Act may be amended or repealed by any Act to be passed in this session of Parliament.

MISCELLANEOUS.

Medical Graduations at Edinburgh.—On the 1st of August 1844, the Senatus Academicus of the University of Edinburgh conferred the Degree of Doctor of Medicine on the following gentlemen, in number sixty-five, after having gone through the appointed examinations, and defended publicly their Inaugural Dissertations.

OF SCOTLAND.

John William Johnston Anderson, on the Action and Curative Properties of Gallic Acid.

William Cameron, on the Blood, and its Morbid Phenomena.

Alexander Fleming, an Historical and Experimental Inquiry into the Physiological and Medicinal Properties of the Aconitum Napellus.

James Henderson, on Intus-susception.

Claud B. Ker, on Muscular Fibre.

Kelburne King, an Historical Account of Stricture of the Urethra, with some Remarks on Fistula in Perinæo.
James Lee, on Tetanus.
Thomas Masson Lee, Observations on Typhus and the Epidemic Fever of 1843.
James Leitch, on Pneumonia.
John Gairdner Lyell, De Vomitu et Remediis Vomitoriis.
John Maitland, on Ulcers—Simple and Specific.
Andrew Scott Myrtle, on Pneumonia, or Inflammation of the Lungs.
Robert Wellbank Macaulay, on the Pathology and Treatment of Dysentery.
Angus M'Leod, on Insanity.
Robert Nicholson, on Pathological Hæmatology.
John Forbes Ogilvie, on Respiration.
David Scott, on the Poisonous and Medicinal Properties of Lead.
John Shand, concerning the Position the Operation at the Ankle-joint promises to hold in regard to Surgery.
John E. Stewart, on Dysentery.
Michael Westall Taylor, on the Pathology of the Urinary Excretion.
Laurence Ramsay Thomson, on the Phenomena and different Forms of Mental Alienation—with a slight sketch of the Mental Faculties in a state of Health.
James Vass, on Erysipelas.
Neville Wood, on the Physical and Chemical Constitution of the Blood.
Richard White Young, on the Signs of Pregnancy.

FROM ENGLAND.

Charles Augustus Adey, on the Scirrhus of Carcinoma, as it usually falls under the eye of the Surgeon.
Alexander Russell Atkinson, on Pneumonia.
John Byrom Bramwell, on Pericarditis.
Frederick James Brown, on the Disorders of Dentition.
Alexander Peile Cahill, on the different views entertained of the Anatomy of the Urethra.
Edward Charles Chepmell, on Dietetic Observances, with a view to the Maintenance of Physical and Mental Health.
William H. Clunie, on Pneumonia.
William Carey Coles, on Epidemic Fevers, intermediate between the Intermittent and Continued Typhus.
Charles Percy Croft, Observations on Cases of Strangulated Hernia.
Frederick Douglas, on Gonorrhœa.
George Grayling, on the Pathological Changes to which the Blood is liable—their Causes and Effects.
William Aurelius Harland, on Spinal Irritation.
Edmund Hutchison, on Tubercular Phthisis.
James James, on Acute Rheumatism.
Frederick Denbigh Jones, on the Phenomena produced by the actions of Narcotic Poisons.
Thomas C. Jones, on Insanity.
Edward John Kennedy, on Inflammation of the Lymphatics.
Charles Hungerford Joseph Berry King, on Pleuritis.
George William Lillies, on Malaria and Miasm.
Charles Palmer, on Strabismus.
William Peach, on Bronchocele—its History and Treatment.
Joseph Pearson, on Epilepsy.
Frank Renaud, on the Ovaries—their Anatomy and Physiology, Comparative and Human.
Adrian Stokes, on Delirium Tremens.
John Richard Wardell, on the Edinburgh Epidemic Fever of 1843.
Thomas Wright, on Ovariotomy.

FROM IRELAND.

Edmund Barry, on Dropsy.
Allan Brown, on Hydrocephalus Acutus.
Patrick Digan, on Pericarditis.
Richard Dyas, on Acute Hepatitis.
William Henry Kent, on Cerebral Apoplexy.
William Leeper, on General Pathology of Nutrition.
Robert Gillman Lord, on the General Structure, Functions, and Diseases of the Vertebral Column

Hugo Pelan, on the Structural Anatomy of the Muscular Tissue and the Phenomena of Muscular Contraction.

Thomas Swan, on Aneurism.

FROM THE EAST INDIES.

Henry Huggins Jones, on Intermittent Fever.

James Cock Watson, on Infanticide.

FROM THE WEST INDIES.

William James Turner Bowerbank, on Intermittent Fever.

William Burt Wright, on Hydrops.

FROM THE ISLAND OF MADEIRA.

Alexander Halley, Observations connected with the Natural History and Climate of the Island of Madeira.

FROM THE ISLAND OF MALTA.

Joseph Alfred Radcliffe Harvey, on the Treatment of Popliteal Aneurism by Modified Compression.

Surgeons' Hall, Edinburgh, 1st September 1844.—Since 31st August 1843, the following gentlemen have offered themselves as Candidates for the Diploma of the Royal College of Surgeons of Edinburgh, and after producing satisfactory evidence that they had completed the Course of Study prescribed by the College, have been admitted to examination, and found fully qualified to practise the arts of Anatomy, Surgery, and Pharmacy, and have received Diplomas as Licentiates of the College accordingly.

Scotland.

Anderson, Geo. Rainy
Brown, Rob. Ebenezer
Baxter, David
Burn, W. Murdoch
Brown, William C.
Christie, Johnstone
Campbell, James
Craig, Robert
Craig, Henry Jamieson
Duncan, George
Flyter, James
Forbes, John
Gordon, James
Irving, James
Lindsay, Alexander
Legat, Andrew
Mitchell, William
Macleish, Alexander
Macintyre, John
Morton, James
Mackay, Duncan
Macnab, James
M'Owan, John
Mackenzie, Patrick M.
M'Leod, Angus
Macaulay, Robert W.
Main, William
Maitland, John
Nisbet, James A.
Ogilvie, John Forbes
Philp, John
Rutherford, Thos. H.
Reid, John Watt
Rae, William
Ritchie, Robert
Small, John
Steele, John Charles
Sharp, William
Scott, David
Smith, John
Sandie, John Gray
Smith, James
Thomson, Laurence R.
Thomson, James
Thomson, William
Thomson, David Purdie
Turner, David
Thorburn, William
Torrance, William C.
Taylor, Michael W.
Wisely, William
Young, James Hume
Young, Richard White

England.

Brown, Frederick Jas.
Cousins, John Sims
Edwards, William
Huish, Henry
Kingdon, Charles Henry
Jones, Thomas C.
O'Brien, Edmund E. H.
Wilkin, George
Woodman, George T.

Ireland.

Bleazby, Samuel
Godfrey, Henry Moore
M'Cullagh, Godfrey
Starret, Samuel
Wigstrom, Robert B.

Abroad.

Bowerbank, W. J. T.
Campbell, Arthur L. S.
Fair, Robert Kendal
Hende, William W.
Harvey, Joseph A. R.
Kennedy, Roderick
Morrieson, James S.
Munro, William
Robertson, Henry F.
Scott, William
Someren, W. J. van
Watson, James Cock
Wells, Samuel S. D.

Extracted from the Records of the Royal College, by

JOHN SCOTT, *Secy.*

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Mr Goodsir's Description of Hydatids

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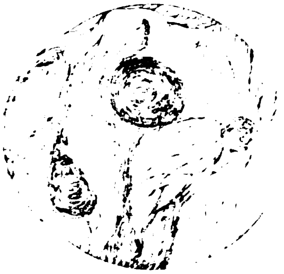

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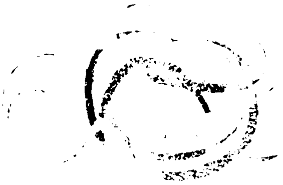

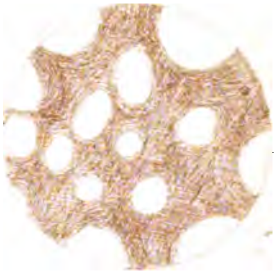

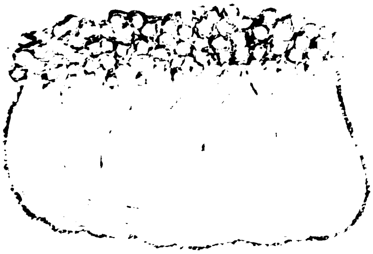

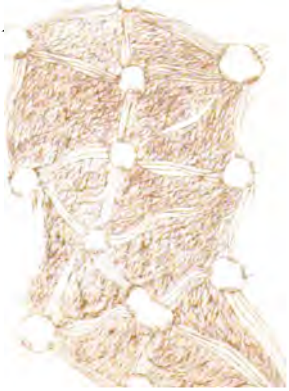

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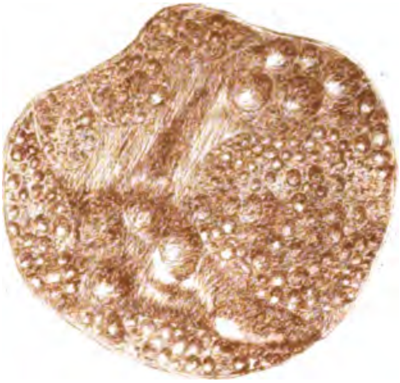

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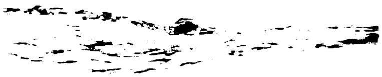

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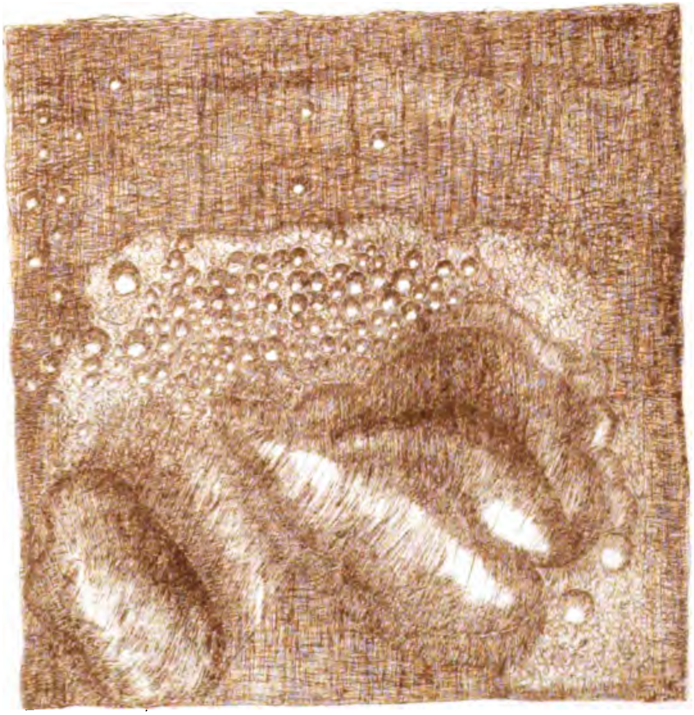

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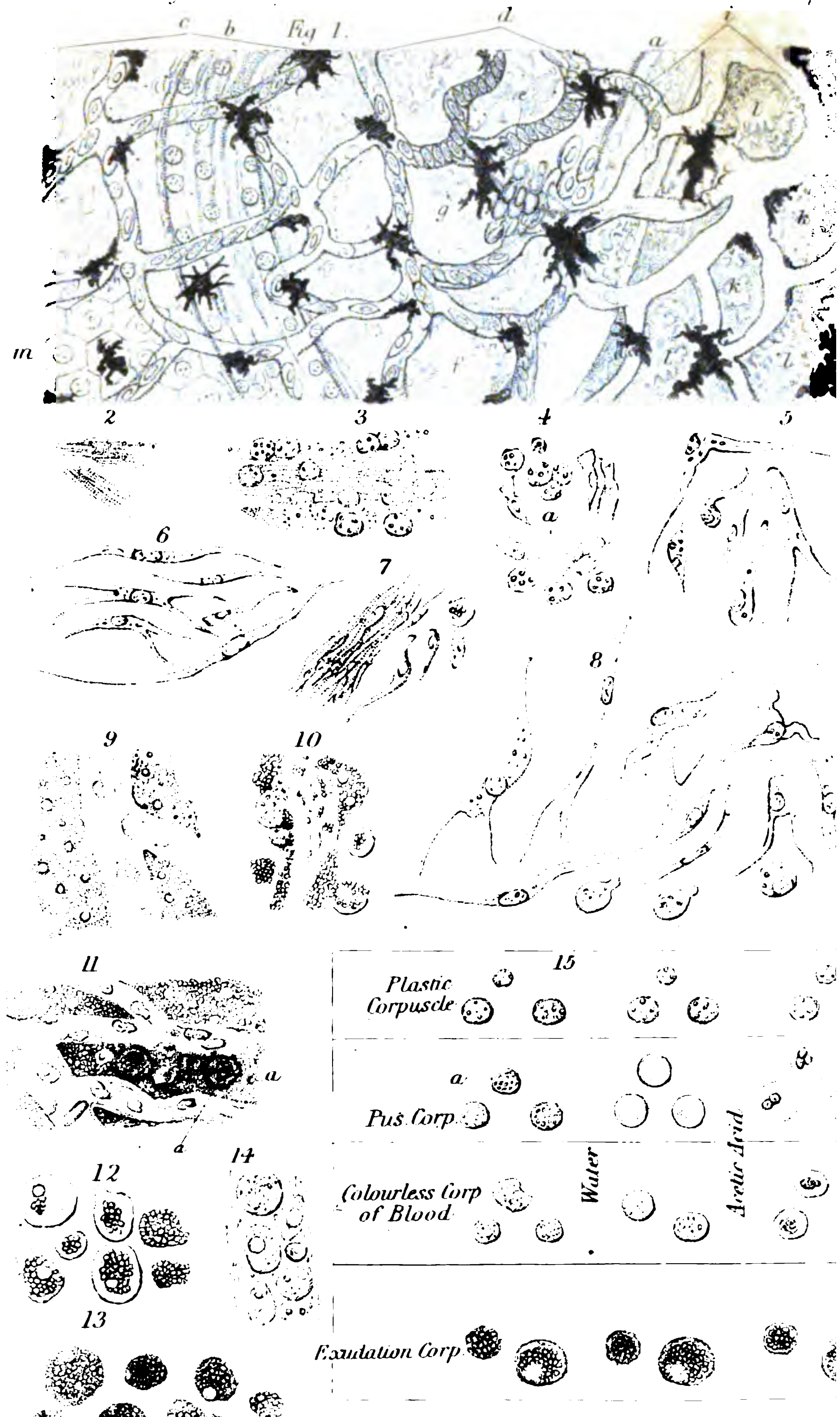

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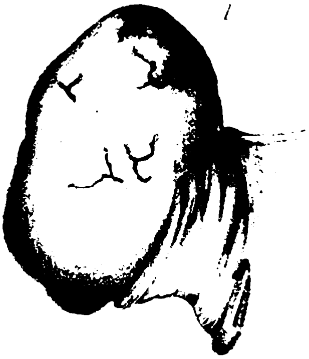

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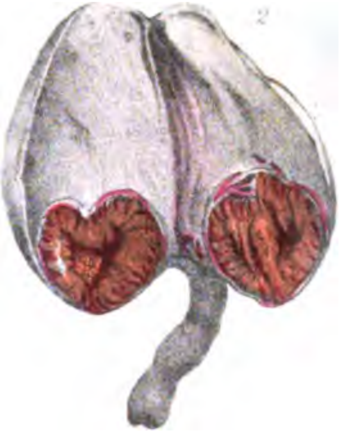

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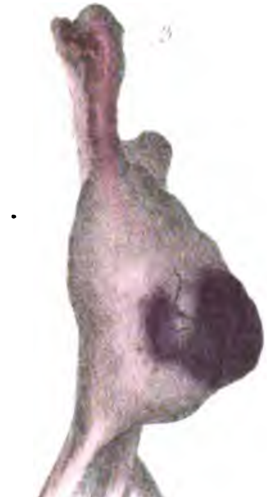

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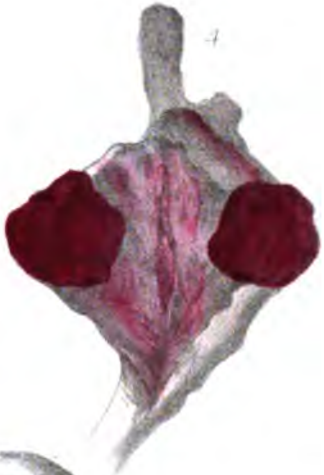

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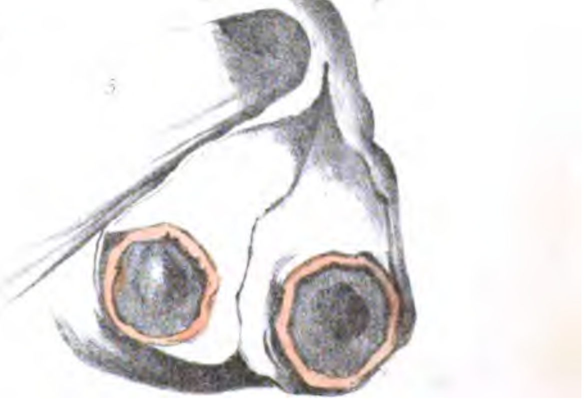

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